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A REPLY TO MR. STEPHEN PAGET'S "EXPERIMENTS ON ANIMALS"

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With an Introduction

By THE HON. STEPHEN COLERIDGE

"How vile an idol proves this god."—*Twelfth Night.*

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PREFACE

For the past twenty-five years the writer has interested himself in the study of the contending claims of those who advocate and of those who oppose experiments upon animals for the advancement of medicine. Altogether apart from the moral aspect of the vivisection question —in which the writer is passionately interested—it has always seemed to him that the scientific side has been so misrepresented by many apologists for vivisection and their dogmatic pronouncements have been so implicitly believed by those who have neither the opportunities nor the inclination to investigate them thoroughly, that in the interests, not less of the medical profession itself than of truth and common honesty, it is incumbent on those who have mastered the literature of the subject to expose the weakness of many of the claims made for vivisection by those who contemptuously assert that their opponents are ignorant and fanatical enemies of science, though possibly more or less amiable and well-meaning persons.

It was therefore with a feeling of satisfaction such as Job would have contemplated his enemy's book had he taken the pains to write one, that he studied Mr. Paget's *Experiments on Animals*—satisfaction that the time had at length come when an eminent authority on medical science had been put forward by his colleagues to answer our case against physiological cruelty, and satisfaction that in a serious and in some respects trustworthy treatise we should have before us the best that could possibly be said on behalf of practices that so large a section of the thinking and humane public abhor. No doubt Mr. Paget has done the very utmost that could be done for what is a bad case —bad on the scientific side and infinitely worse on that of ethics. In the following pages Mr. Paget's examples are taken one by one and his contentions tested by historical facts and by the utterances of experts who have surveyed the same fields of observation, and for the most part by reference to standard works on medicine, surgery, and materia medica of unimpeachable authority. Mr. Paget says that his only way of answering us was "to give the original authorities, the plain facts, the very words, chapter and verse, for everything." But the work of those authorities has been overhauled times without number by other authorities equally eminent in their own departments of medical science, and their results have, above all, been tested by clinicians whose business it has been to test at the bedside the value of the lessons imparted from the laboratory. Mr. Paget has not based his contentions on the ground of advantage to abstract science, but on that of utility to practical medicine. On this basis the answer contained in the following pages is built up. To learn by cruel experiment what nerves influence the wagging of a dog's tail or the arching of a cat's back does not seem very important to us as doctors. What is the cause of and remedy for cancer, for diabetes, for plague, and yellow fever, on the other hand, is of the utmost importance to the practitioner of medicine, and how far experiments on animals have helped them in dealing with such maladies is a legitimate subject for unbiassed inquiry such as is pursued in the present treatise.

LONDON, *April* 4, 1903. EDWARD BERDOE.

INTRODUCTION

Lord Lister, in introducing Mr. Stephen Paget's volume, written to defend the practice of vivisection, announced to the world that Mr. Paget's work was "entirely a labour of love." But the perplexed public were not informed what was the specific object of Mr. Paget's affection. It was surely no love of the vivisected animals that directed his pen, nor could it have been the love of men, or he would certainly have uttered some words of pity for Sanarelli's human victims. Perhaps it was love of vivisection in itself, as an occupation and pastime, for which Mr. Paget laboured, and in that case he must be assumed to have no sympathy with those few vivisectors who have sometimes spoken of the cutting up of live animals as a disagreeable necessity.

But whatever Lord Lister may have meant by the phrase, there can be no doubt what are the motives that inspire those against whom he and Mr. Paget inveigh. Their motives find their source in the most sacred principles of humanity, and in the deepest foundations of religion. They earnestly believe that to take such a faithful, loving creature as a dog, to cut it and carve it alive, taking out one of its kidneys piece by piece, keeping it alive for many days, suffering from thirst, vomiting, and ulceration till it was thought well to remove the entire kidney in a similar manner on the other side, is to do to that dog what is appalling to any brave man or gentle woman, is to degrade mankind far, far below the poor mangled victim, and is to offend the justice, the mercy, and the majesty of God.*

They care not whether some benefit be claimed for man's body by such acts, for they are convinced that such acts must blast his soul.

If in this volume, the result of much learning and industry, Mr. Berdoe should dissipate even the selfish claims and confute even the squat materialism of these delvers into living dogs, he will have served a worthy part in this great combat, and, as a fellow-soldier, with all my heart I bid him God-speed!

STEPHEN COLERIDGE.

* *The Journal of Physiology*, vol. xxiii. No. 6, contains a record of some experiments on the kidneys of dogs performed at the Brown Institution. The object was to discover how the animals could sustain life, and for how long, with portions of their kidneys cut out from time to time. Forty-nine dogs were used in this research. One of them died on the fourth day after the operation, another on the sixth, a third thirty-six days afterwards. Then twenty-eight of the dogs were used for a second operation—the removal of the entire kidney on the opposite side.

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PART I

I

HARVEY AND THE CIRCULATION OF THE BLOOD

The popular notion of a "discovery" is that it is something sudden, the work of a man who, in an inspiration of genius, hits upon something of importance to the race. Thus we heard in our young days a legend to the effect that James Watt, watching his mother's tea-kettle on the fire, observed that the steam lifted the lid as it escaped; that Isaac Newton, seeing an apple falling from the tree, was inspired with his theory of gravitation. In like manner the uninformed believe that Harvey, having vivisected a great number of different animals, "discovered" the circulation of the blood. To many persons it is new that the invention of the steam engine has occupied the thoughts of mechanical thinkers for many hundreds of years.

Hero of Alexandria, who flourished about 284–241 B.C., describes in his *Pneumatics* various methods of employing steam as a power, and to him is ascribed the Æolopile, which we should now call a mere toy, yet it possesses the properties of a steam-engine, and Roger Bacon seems to have foreseen the application of steam power.

Seneca, about 38 A.D., speaks of the moon attracting the waters, Kepler investigated the subject about 1615, Hooke worked out a system of gravitation about 1674, and Galileo about 1633 demonstrated its principles, but in 1666 Newton saw the apple fall, set to work on the subject and the theory was established. The discovery of the circulation of the blood was not the work of one man nor of one year. Mr. Paget, in his opening paragraphs on the blood, tells us that Galen, (born 131 A.D.,) proved by experiments on animals, that the arteries during life contain blood, and not air, thus disproving the teaching of Erasistratus that the arteries contain the breath of life. Now Erasistratus was one of the most famous experimental physiologists of the Alexandrian School, who vivisected criminals handed over to him for the purposes of research. The ancient apologists of this human vivisection said, "it is not to be called an act of cruelty, as some persons suppose it, to seek for the remedies of an immense number of innocent persons in the sufferings of a few criminals." Erasistratus flourished B.C. 340–280, was a competent physiologist for his time, had a "free vivisecting table," yet fell into the gross error which Galen by more vivisection corrected. Even in Plato's time (B.C. 427–347) the idea of a circulation was held, for that philosopher says in the *Timaeus* :—

> "Now after the directing artificers of our structure had implanted all these organs for giving nourishment to our inferior nature, they directed various channels through our body, so as to water it like a garden, by the constant accession of flowing moisture . . . and that the flood (of blood) supplied thence to other parts might give an equable irrigation."

Near the end of the fourth century Nemesius, Bishop of Emissa, in Phœnicia, described the circulation in curiously plain terms. He says:—

> "The motion of the pulse takes its rise from the heart, and chiefly from the left ventricle of it; the artery is, with great vehemence, dilated and contracted by a sort of constant harmony and order. While it is dilated it draws the thinner part of the blood from the next veins, the exhalation or vapour of which blood is made the aliment for the vital spirit. But while it is contracted it exhales whatever fumes it has through the whole body and by secret passages. So that the heart throws out whatever is fuliginous through the mouth and the nose by expiration."[1]

The scientific Bishop's book, *On the Nature of Man*, served as a physiological text-book for many ages. The father of modern anatomy was Mondino, who taught in Bologna about the year 1315. His anatomy of the heart is wonderfully accurate, and he came very near to the discovery of the circulation of the blood. The glorious sixteenth century with its reformation of medicine dawned in Italy, and produced a host of anatomists who accumulated a mass of facts which laid the foundation for Harvey's work. Vesalius (1514–1564), by his study of the heart and the mechanism of its valves set his pupils and fellow students on the path that ended in Harvey's discovery. Vesalius contradicted Galen's statement that the arteries contained blood and not air—Vesalius was a vivisector; his works contain plates representing quadrupeds of all sorts tied up evidently awaiting vivisection.[2] If

[1] Friend, *Hist. Med.* .

[2] See statement by Dr. Molony in *British Medical Journal* December 31, 1892.

Galen by vivisection proved—as Mr. Paget says—that the arteries during life contain blood and not air, we are equally safe in asserting that Vesalius was misled by his vivisections in believing the contrary. He taught that while the arteries were merely the conductors of the vital spirits from the heart throughout the body, the veins were the principal vessels. Michael Servetus (1511–1553) was either a pupil or a fellow-student of Vesalius, and he in 1553 described accurately the circulation of the blood through the lungs. He discovered that the change from venous into arterial blood took place in the lungs, and not in the left ventricle of the heart. This was a discovery of enormous importance. Hallam says[1] that one Levasseur, about 1540, appears to have known the circulation of the blood through the lungs, the valves of the veins, and their direction and purpose.

Columbus (who died in 1559), a pupil of Vesalius, was a vivisector who was the first to perform experiments on dogs in place of hogs, because the latter annoyed him by their squealing, yet with all his research he could not get rid of the idea of the "vital spirit" conveyed by the arteries. He demonstrated the fact that the blood passed from the lungs into the pulmonary veins, that it pushed on further into the left ventricle, and so established the lesser circulation from one side of the heart to the other. Yet his vivisection did not even show him the truth about the function of the arteries. The Italians maintain that Caesalpinus (1519–1603) was the discoverer of the complete circulation. Professors Mazziorani and Bacelli of Rome in 1876 even went so far as to erect a monument to his memory as the discoverer of the fact. True, he is said

[1] *Lit. of Europe*, chap. ix., sect. 2.

to have been the first to use the term *circulatio*, but he did not know of the direct flow of the blood from the arteries to the veins.

Concerning all these men, Mr. Paget says (p. 6) :—

> "The work that they did in anatomy was magnificent; Vesalius, and the other great anatomists of his time, are unsurpassed. But physiology has been hindered for ages by fantastic imaginings and the facts of the circulation of the blood were almost as far from their interpretation in the sixteenth century as they had been in the time of Galen."

But Mr. Paget omits to remind his readers that all these anatomists were also vivisectors. If "physiology had been hindered for ages by fantastic imaginings," why did not their countless vivisections correct their fancies? From the Alexandrian experiments, through the later Roman times, and those of the Renaissance, both living men and animals were freely opened in the laboratories by the anatomists, yet they all missed the truth concerning the circulation. It certainly, to say the least, does not prove much as to the value of vivisection in relation to the circulation, if two thousand years of animal torture did not suffice to settle the question of the function of the arteries, so that we may be pardoned if we attach little importance to the practice now. But to proceed :—

In the year 1574 Fabricius of Aquapendente, a pupil of Fallopius and the teacher of Harvey, observed valves in most of the veins of the human body. The history of their discovery is quite obscure, though the priority of observation is generally ascribed to Fabricius. Several other anatomists had previously described them, but it was certainly he who made drawings of them and rightly explained their use. He said they were designed by nature to prevent congestions and the too great dilatations of the veins. "In the arteries," he said, "the valves are unnecessary, as the influx and reflux of the blood here is not so much interrupted as in the veins." Now we know that Fabricius demonstrated the valves to Harvey at Padua, "and it is probable," says Professor Flint,[1] "that this was the origin of the first speculations by Harvey on the mechanism of the circulation." Harvey published his celebrated work in 1628; his predecessors in the line of research which he followed so successfully were, as we have seen, Servetus, who died in 1553, and Columbus, who died in 1559, both these anatomists had established the lesser circulation of the blood through the lungs by anatomical observance alone. Vesalius, dying in 1564, had, by his study of the heart and the mechanism of its valves, smoothed the path for Harvey concerning the movements of the organ, and Caesalpinus, who died in 1603, had, so long ago as 1570, shown in his treatise *Speculum Artis Medicae Hypocritum*, that he had at least a general knowledge of the circulation. He says:—

> "In animals we see that the nutriment is carried through the veins to the heart as to a laboratory, and its last perfection being there attained, it is driven by the spirit which is begotten in the heart through the arteries, and distributed by the whole body." [2]

There are two distinct circulations by which the blood can pass from one side of the heart to the other. One is the pulmonary circulation by which it passes

[1] *Human Physiology*, p. 30. [2] *Op. cit.*, Lib. I., Cap. II.

from the right side to the left through the vessels of the lungs. We call the other the systemic circulation, or the circulation through the whole of the body. This begins at the left side of the heart in the ventricle, and ends at the right side at the auricle. Of course the pulmonary circulation is not nearly so extensive as the systemic, and so is called the lesser circulation. yet is of immense importance, as by the passage of the blood through the lungs it is oxidized, and the dark venous, impure blood becomes changed into the bright red arterial blood. We see then that eight years before Harvey was born all the facts about the lesser circulation had been observed and published, and that nearly a hundred years before his time all the anatomical facts necessary for Harvey's discovery had been accurately described. One master mind was needed to collect the rays from all these sources of light into one focus.

Then came Harvey, as the master mind always does come, when the way is duly prepared before him. Harvey was a great vivisector. Mr. Paget gives us several pages from his works and proves what nobody has ever disputed. It is perfectly true also that Harvey again and again, in the plainest terms, declares that his experiments on living animals aided him in his discoveries. But that is not so important as it appears to be. We can seldom decide what precise set of circumstances have brought about a definite course of action in reference to ourselves. We have seen how much of the ground had been prepared for Harvey by the anatomists who preceded him, we know from his own work how much he owed to anatomy of the dead subject, how splendid was the reasoning he brought to bear on the mechanism of the heart and blood vessels, altogether apart from anything which the opening of living animals could possibly have taught him. We know by the emphatic testimony of a great surgeon like Mr. Lawson Tait that "the circulation of the blood could neither be discovered nor demonstrated now by anything but a dead body and a syringe,"[1] and above all we have the testimony of the Hon. Robert Boyle, how Harvey, in his later life, told him that it was the arrangement of the valves in the veins (which could only have been learned on the dead subject) which led to the discovery of the circulation :—

> "I remember that when I asked our famous Harvey, in the only discourse I had with him, which was but a while before he died, what were the things which induced him to think of the circulation of the blood, he answered me that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave free passage of the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was invited to imagine that so provident a cause as Nature had not so placed so many valves without design ; and no design seemed more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent by the arteries, and return through the veins, whose valves did not oppose its course that way."

This seems conclusive as to one point, but Mr. Paget, in the first edition of his book, discounts the testimony by reminding his readers that it was given "but a while before he (Harvey) died," and that Harvey lived to

[1] *The Uselessness of Vivisection*, p. 6.

four score years, "an old man far advanced in years and occupied with other cares," as he spoke of himself. "It is stupid, or worse than stupid, to attempt to set this letter against Harvey's exact words," says Mr. Paget.[1] It is satisfactory to note that in his "New and Revised Edition" (1903) the writer has been content to state the facts without intemperate comment.

Because Harvey was advanced in years when he told the Hon. Robert Boyle how he gained his idea of the circulation is very far from indicating that his mind was too enfeebled to enable him to do so correctly, rather does it imply that in the calm repose of life, after the heat of battle, and the storm and stress of discussion have subsided, the real and clear state of the matter would remain undisturbed in his mind. That there is no "stupidity" in the contention is shown by the testimony of unprejudiced physicians at the present day. Thus, Dr. Acland before the Royal Commission,[2] said :—

> "It is not quite certain what argumentation led Harvey to that (the circulation), whether it was the observation of the living structure or the contemplation of the dead structure."

Dr. Acland was Regius Professor of Medicine in the University of Oxford and of course spoke with authority.

Sir Thomas Watson, one of the physicians in ordinary to Queen Victoria, and a President of the Royal College of Physicians, examined before the Royal Commission, was asked (Q. 84), "You were saying that the circulation

[1] *Experiments on Animals*, by Stephen Paget, p. 11 (1st ed. no date).

[2] *Blue Book*, Q. 991.

of the blood has been discovered by Harvey by vivisection. Is it not true that he was led to it by a great many other observations before he verified it in that mode; that it was a verification rather than a discovery, in fact?"

Sir Thomas replied: "Yes; but I think the discovery was rendered perfect by his vivisections."

Mr. George Macilwaine, F.R.C.S., was also examined on the matter; he said (Q. 1845–6):—

> "I find that the discovery of the circulation of the blood is referred to vivisection. In the first place, any man who knows what the circulation is will see that intrinsically it could not be; you do not want the authority which is suggested to you, because you could not discover the circulation in the living body; I do not see how it is possible to do it. If you had a dead body, then it is so easy to discover the circulation, that it is difficult to understand how it was not done before; because if you inject by the arteries you find that it is returned by the veins."

Dr. J. H. Bridges, of the Local Government Board, delivered the Harveian Oration on October 20, 1892, at the Royal College of Physicians. He said:—

> "It is sometimes said that experimentation on living animals was the principal process of (Harvey's) discovery. This I believe to be an exaggerated view, though such experiments were effective in convincing others of the discovery when made. It need not be said that no ethical problem connected with this matter was recognized in Harvey's time."

Although Harvey is credited with having discovered the circulation, it is the fact that whatever was done in this direction before his time, the complete circulation was not discovered and made known until thirty-three years after the publication of Harvey's great work, *De Motu Cordis*. By "complete" I mean the manner in which the blood makes its way from the arteries into the veins, This is known as the capillary circulation. The minute blood vessels connecting the arteries and veins were discovered by Malpighi and described by him in 1661, in the lungs and mesentery of frogs by means of the microscope invented in 1621. It is true that in this observation he used a vivisectional experiment, which was not in the least necessary, for, as Mr. Lawson Tait said,[1] "he could have better and more easily have used the web of the frog's foot than its lung." Every student of physiology observes the circulation in this manner now, and as it causes the frog no pain whatever, it is a perfectly legitimate mode of research.

The Blood Pressure

The statics and dynamics of the blood pressure, that is to say, the quantity and swiftness of the blood in its passage through the vessels, have occupied the attention of vivisectors for a hundred years and more, and the research is still being pursued in the laboratories. Mr. Paget says the experiments of Keill on the blood pressure (1718) "were inexact and of no value, and the first exact measurements were made by Stephen Hales," who published his work, entitled *Statical Essays*, in 1726–1733. But Professor Flint says[2] concerning

[1] *Uselessness of Vivisection*, pp. 3, 4.
[2] *Human Physiology*, p. 72.

the experiments on the carotid of a living horse, performed by this cleric:—

> "The experiments of Hales were made with a view of calculating the force of the heart, and were not directed particularly to the modifications and variations of the arterial pressure. It is only since the experiments performed by Poiseuille with the hæmadynamometer, in 1828, that physiologists have had any reliable data on this latter point."

Again he says:—[1]

> "Physiologists have only an approximate idea of the arterial pressure in the human subject, derived from experiments on the inferior animals."

How worthless is such an approximate idea may be gathered from the following remarks by A. Morrison, M.D. (Ed.), F.R.C.P. (Ed.), in the *British Medical Journal*, March 14, 1896, p. 650:—

> "From the days of Stephen Hales (1728) until now, the relations between artery and manometer have not been such as obtained in Nature, and the argument from quadrupeds to man as to the power of the heart and pressure in the blood-vessels are fallacious, for those physical reasons which bring about a difference in the circulatory apparatus of animals habitually and respectively horizontal and erect."

The experiments of Hales, Poiseuille, Claude Bernard and others seemed to prove that the regular arterial pressure varied little in different sizes of arteries,

[1] *Op. cit.* p. 73.

but their experiments had too narrow a range, and Volckmann found that this was incorrect, for he found that the pressure is greater in the arteries nearest the heart and gradually diminishes towards the capillaries. Despite all these experiments, no practical results seem to have accrued to practical medicine.

The force of the heart varies in different animals, and under different conditions, and we have other and more useful means of ascertaining the facts than by cruel experiments on animals.

Hunter and the Collateral Circulation

If we tie a main artery the blood vessels in its neighbourhood become enlarged and the parts beyond the ligature are supplied with blood by what anatomists call "the collateral circulation." Mr. Paget attributes John Hunter's discovery to his experiments on animals. He tells us how Hunter conceived and performed his operation for aneurism (December, 1785), and that he got this knowledge from the experiments that he made on a deer. Now, Hunter only improved on an older method of applying ligatures for aneurisms, this was Anel's method in which the artery was tied just above the swelling on the side nearest the heart; it proved dangerous, because the artery near the seat of the disease would not hold the ligature and the patients bled to death. On this important subject Mr. Lawson Tait says :—[1]

> "As the arteries of animals never suffer from the disease in question (aneurism) experiments upon them could not have helped Hunter in any way whatever."

[1] *Uselessness of Vivisection*, p. 22.

Sir James Paget, the father of Mr. Stephen Paget, has recorded his opinion in the Hunterian Oration given at the College of Surgeons in 1877, that Hunter's improvement in the treatment of aneurism

> "Was not the result of any laborious physiological induction; it was mainly derived from facts very cautiously observed in the wards and dead house."

We know that Hunter tried his best to induce aneurism in animals and failed. Hunter's patient, upon whom he first operated by the new method, died about a year afterwards. At the post-mortem examination the manner in which the blood found its way to the parts below the ligature could be made out by careful dissection.

Registration of the Blood Pressure

There is a delicate instrument for recording the pulse-wave which is called the "sphygmograph." This was improved by a physiologist named Marey, and Mr. Paget has devoted a section of his chapter on the blood to its explanation. The instrument itself is applied to a part of the body where the pulsations of an artery can be felt, and its invention involved no vivisection whatever, and no experiments on animals and human beings which occasioned pain. Mr. Paget does not tell us this, and the unprofessional reader might be led to imagine that the invention of the apparatus was due to vivisection. Whatever its interest to scientists, its application is not so easy as to make it generally useful in the practice of medicine,[1] although

[1] Flint's *Human Physiology*, p. 68.

when skilfully used the instrument gives on paper the actual "form of the pulse." The *Text-Book of Medicine*, by Drs. Fagge and Pye-Smith sums up[1] the account of the apparatus thus :—

> "But on the whole, the expectations raised by Marey's instrument have not been realized ; and the sphygmograph is rather useful as a corrector or confirmer of a diagnosis based on other grounds, than a discoverer of unexpected lesions, like the stethoscope or laryngoscope."

Another instrument, of a somewhat similar sort, described by Mr. Paget, is the cardiograph of Chauveau and Marey for the observation of the blood pressure within the cavities of the heart. The use of this involves making an incision through the chest of the animal experimented upon just over the point where the beating of the apex of the heart is felt,

> "a little bag, stretched over the metallic buttons, separated by a central rod, is then secured in the cavity thus formed and is connected by an elastic tube with the registering apparatus." [2]

We are told, in physiological text-books, that in this experiment "the animal experiences no inconvenience, is able to walk about, eat, etc."

Those who tell us this seem to have copied the words of Marcy, the inventor, who says the animal so treated "is in no wise disturbed, walking and eating as usual," which may be advantageous for the experimenter to say, but not necessary for us to believe. To pierce

[1] Vol. ii. p. 169.
[2] Flint's *Human Physiology*, p. 42.

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the heart and push tubes into its cavitics without rcsorting to any form of anaesthesia would strike most people as an experiment calculated to cause acutc pain.

II

THE LACTEALS

The lacteals are the lymphatics of the small intestine that take up the chyle. They were discovered by Asellius in 1622, and, as Professor Flint says, the discovery "is more interesting in an anatomical than in a physiological point of view." From the time of Hippocrates, the absorbent vessels were vaguely alluded to, yet they were not recognized. Mr. Paget quotes Asellius as saying :—

> "As for Galen I know not at all what I am to think. For he, who made more than six hundred sections of living animals, as he boasts himself, and so often opened many animals when they were lately fed, are we to think it possible that these veins never showed themselves to him, that he never had them under his eyes, that he never investigated these—he to whom Erasistratus had given so great cause for searching out the whole matter?"

What are we to think? Why, surely that vivisection cannot be the high road to physiological discovery. If Galen, with his six hundred vivisections, failed to interpret properly what he must have seen so frequently, can we wonder at the confusion into which the opening of living animals has thrown medical and surgical science? Asellius made his discovery by accident, and vivisection was in no way necessary. That he was in the act of vivisecting a dog when he made it is not in dispute, but nothing was gained by the fact that the animal was living when he saw the threads which he first thought were nerves. Sir Charles Bell tells us[1] how to find the lacteals:—

> "When the young anatomical student ties the mesenteric vessels of an animal recently killed, he finds the lacteals gradually swell; he finds them turgid if the animal has had a full meal, and time has been afforded for the chyle to descend into the small intestines; he finds them empty or containing only a limpid fluid, if the animal has not had food. When he sees this he has had sufficient proof that these are the vessels for absorbing the nutritious fluids from the intestines. The actual demonstration of the absorbing mouths of the lacteal vessels is very difficult. The difficulty arises from these vessels being in general empty in the dead body, from the difficulty of injecting them from trunk to branch, in consequence of their valves; and lastly from their orifices never being patent, except in a state of excitement. The anatomist must therefore watch his opportunity when a man has been suddenly cut off in health and after a full meal. Then the villi of the inner coat may be seen tinged with chyle, and their structure may be examined."

This has been done: Cruickshank opened a woman

[1] *Lectures*, p. 360.

who had died suddenly of convulsions after taking a hearty supper in perfect health. Notwithstanding all these facts, which of course are common knowledge, Mr. Paget says (p. 27): "On the dead body, lacteals, receptaculum, and thoracic duct would all be empty." The wealthy medical amateur, Fabrice de Peiresc (1580–1637) by the investigation of the body of a highly fed malefactor two hours after his execution discovered the vessels in question in man.[1] Mr. Paget surely knows these things, yet to make the best of his case for the vivisectors, says nothing about them; this is hardly "playing the game." Yet, further, although vivisection by chance showed Asellius the lacteals, Bartholini, Magendie and others disproved the errors into which Asellius and Pecquet fell in consequence of further experiments on animals as to the direction of the contents of the lacteals. They thought that

> "the lacteals absorb all the products of digestion; . . . it is now known that fats, in the form of a very fine emulsion, are absorbed by the lacteals and that these are the only constituents of food taken up in great quantity by this system of vessels." [2]

[1] Baas, *History of Medicine*, p. 533.
[2] Flint's *Human Physiology*, p. 273.

III

THE GASTRIC JUICE

Mr. Paget has a good deal to say about experiments upon animals in connexion with the study of digestion. He gives us a long account of the history of this research taken from Claude Bernard's *Physiologie Operatoire* (1879). However interesting as historical physiology, none of the experiments on animals was of any importance to practical medicine. Experimenters, as is ever their custom, contradicted one another, and such facts as were demonstrated by them could have been learned by simple observation without vivisection. It is of very little importance to the physician to know how birds of prey triturate food in their gizzards, or how the gastric juice of dogs can digest bones. What is of importance to doctors is the study of digestion and the constitution and power of the gastric juice in human beings. Dr. Noel Paton[1] tells us that—

> "Various opportunities have occurred, and have been taken advantage of, to study the interior of the human stomach during life. The last-known investigation of the kind was undertaken by Dr. Beaumont, a Canadian physician, on the

[1] *Essentials of Physiology*, 1903, p. 300.

> person of St. Martin, a backwoodsman, who had received a gun-shot wound in the abdomen, which had left him with an opening through the front wall of his stomach. Dr. Beaumont engaged St. Martin as his servant, and made a prolonged and valuable study of the changes which take place in the viscus."

This Dr. Beaumont (1825) was the first to obtain human gastric juice through the opening in St. Martin's stomach, which was partially covered with a fold of skin. He introduced various articles of food, and noted the time required for their solution. It was by these apparently painless experiments on a voluntary human patient that we have obtained the approximate time needed for the digestion of some principal foods. We know that Spallanzani, Stevens, Tiedemann, Gmelin and others obtained the gastric fluid of dogs by causing them to swallow pieces of sponge which were subsequently withdrawn soaked with the fluid. We know that Blondlot, Bernard and others performed similar experiments, but, as Dr. Kirkes says,[1] "These need not be particularly referred to while we have the more satisfactory and instructive observations which Dr. Beaumont made with the fluid obtained from the stomach of St. Martin." Let us compare the quantity of the gastric juice secreted in twenty-four hours in the man as estimated by Beaumont from his observations on St. Martin with the observations on dogs made by Bidder and Schmidt. Beaumont's patient only secreted 180 grams daily, whilst the dogs yielded $6\frac{1}{2}$ kilos daily, corresponding to $\frac{1}{10}$ of the body weight. The free hydrochloric acid

[1] *Handbook of Physiology*, 8th ed., p. 277.

in the gastric juice, which is so powerful a solvent of the food, is stated by *Prout* (1824) as 0·2 to 0·3; *Richet*, 0·8 to 2·1 per 1,000; in the dog, 0·52 per cent.[1] The analyses vary in different books, but we may accept the statement of Dr. Noel Paton[2] that "in the dog the free acid may amount to 0·2 per cent., but in man it is less abundant," as proving that the digestive powers of man are not quite identical with those of the dog, who has suffered so many and great tortures in this line of research. Without detailing the observations of Dr. Beaumont, we may say that it was from them that we have practically obtained what we find recorded in medical books as to the time required for digestion in our own stomachs. The following table from Wellcome's *Medical Diary* gives some of these particulars:—

APPROXIMATE TIME NEEDED FOR THE DIGESTION OF SOME PRINCIPAL FOODS.

Beef, boiled	3 hours.
Beef, roasted	3 to 4 hours.
Fish, boiled	1½ to 2½ hours.
Lamb	2½ hours.
Mutton, boiled	3 hours.
Mutton, roasted	3 to 3½ hours.
Pork, roasted	5 hours.
Poultry, boiled or roasted	2½ to 4 hours.
Tripe	1 hour.
Veal (as prepared in the British Isles)	4½ hours.
Eggs, raw	1½ hours.
Eggs, fried or boiled hard	3 to 3½ hours.
Cheese	3 to 4 hours.
Apples	3 to 4 hours.
Cabbage	3½ to 4 hours.

[1] *A Text-Book of Human Physiology*, Landois and Stirling, 3rd ed., p. 242.

[2] *Essentials of Human Physiology*, p. 300.

Carrots	..	3 to 3½ hours.
Potatoes	..	2½ to 3½ hours.
Turnips	..	3½ to 4 hours.
Rice	if completely cooked	1 to 2 hours.
Sago	if completely cooked	1 to 2 hours.
Tapioca	if completely cooked	1 to 2 hours.
Wheaten Bread	..	3 to 4 hours.

On the question of the gastric juice, Mr. Paget's omissions are very remarkable. He tells his readers nothing whatever about the disputes between various experimenters, French and German, as to the results of their researches on digestion. So acute are they that M. Metzger asks,[1] "Why are you not here, O Molière, to flog with your rod of iron, your powerful irony, all these false savants, all this false science, which deludes the gaze of the vulgar, as you did to those of your own time?"

Is the secretion of the gastric juice continuous or intermittent? From the empty stomach we may say it is absent. When foreign bodies, as pebbles, are introduced, some experimenters say that gastric juice appears; others deny this, and say that only mucus, or an acid liquid, more or less inactive, is present. The true gastric juice, which aids digestion, only appears, say others, when food is introduced. But the majority of German physiologists deny the existence of an acid gastric juice, which is non-digestive.

Mr. Paget tells us (p. 34) that—

> "It has been said, times past number, that an animal with a fistula is in pain. It is not true. The case of St. Martin is but one out of a

[1] *La Vivisection, ses dangers et ses crimes*, p. 148.

multitude of these cases; an artificial orifice of this kind is not painful."

Now this sentence is Jesuitical. It is possible for a human being, humanely treated, to suffer little or no pain from a gastric fistula such as St. Martin's; but in the physiological laboratory, with a helpless animal victim, things are not quite so pleasant. M. Schiff, the vivisector, performed a great number of vivisections on dogs in his researches on digestion, and he says (*Digestion*, tom. ii. p. 243) that his experiments with the fistulas of the stomach which he produced in animals were operations "which always caused a pretty sharp pain to the animals."[1]

Again, in his *Physiologie de la Digestion* (tom. ii. p. 368) he says that the dogs, on the sensibility of whose stomachs he was experimenting, "showed evident signs of pain" (*des signes évidents de douleur*).

[1] "Pour en empêcher la cicatrisation (du susdit canal), j'ouvrais la fistule de temps en temps, trois à quatre fois par semaine, je saisissais le fil avec une pince, et je lui imprimais quelques mouvements de va-et-vient, *opération qui causait toujours une assez vive douleur à l'animal.*"—Tom. ii. pp. 301, 302.

IV

GLYCOGEN: CLAUDE BERNARD AND DIABETES

Mr. Paget, like most apologists for vivisection, depreciates all physiological and pathological research which does not depend upon experimentation on animals. With regard to that function of the liver by which sugar is formed and glycogen is stored therein, he says, "Before Claude Bernard the pathology of diabetes was almost worthless." To read the laudation of Bernard's discoveries in this connexion in Mr. Paget's book the uninformed would be likely to conclude that the results of his researches had never been disputed by other experimenters. He dismisses in three lines at the end of the chapter on Glycogen the researches of Dr. Pavy, remarking that "In England especial honour is due to Dr. Pavy for his lifelong study of this complex problem" of diabetes and the glycogenic functions of the liver. Let us see what Dr. Pavy has done in this connexion. In his *Text-Book of Human Physiology*, ed. 4, pp. 409–10, Professor Flint says:—

> "It is almost certain that the liver does not contain sugar during life. Many years ago (1858) this fact was recognized by Pavy, and it has since been confirmed by other physiologists. Pavy,

however, assumed that there was no such thing as sugar-formation by the liver under absolutely normal conditions. He regarded the sugar found in the substance of the liver and in the blood of the hepatic veins as due to post-mortem action, and his observations seemed to be directly opposed to those of Bernard. The views of these two observers and their followers seemed to be harmonized by a series of experiments made in 1868. If the abdomen of a dog, perfectly quiet and not under the influence of an anæsthetic, be opened, and a portion of the liver be excised, rinsed in cold water, and rapidly cut up into boiling water, the extract will show no reaction with Fehling's test for sugar. In one experiment, in which twenty-eight seconds elapsed between the time of opening the abdomen and the action of the boiling water, the reaction with Fehling's test was doubtful. In an experiment in which the time was only ten seconds, there was no trace of sugar in the extract from the liver (Flint). Dalton, however, in 1871, found small quantities of sugar in extracts of portions of liver taken from an animal in an average time of six and a quarter seconds; but it is possible that the sugar may have been in blood retained in the liver. All observers, however, are now agreed that sugar is formed in the liver very rapidly after death."

Claude Bernard's pathology of diabetes, it must be remembered, was opposed by Pavy, McDonnell, Meissner, Ritter and others. In Quain's *Dictionary of Medicine* (1883), p. 347, we read that—

"Many pathological conditions have been recorded as occurring in those who had been the subjects of diabetes, but we know little of its real pathology."

Mr. Paget refers (p. 39) to Bernard's further discovery that this formation of sugar by the liver is increased by puncture of the floor of the fourth ventricle of the brain. This was published in 1849. He does not tell us how the experiment was performed. Professor Flint describes it.[1]

> "The operation is not difficult. The instrument used is a delicate stilet, with a flat, cutting extremity, and a small projecting point about $\frac{1}{25}$ of an inch (1 mm.) long. In performing the operation upon a rabbit, the head of the animal is firmly held in the left hand, and the skull is penetrated in the median line, just behind the superior occipital protuberance," etc., etc.

After a few technical details, we learn, with some surprise, that "this experiment is almost painless," and then, with no surprise at all, that "it is not desirable to administer an anæsthetic, as this, in itself, would disturb the glycogenic process." Professor Flint informs us that the production of diabetes in this way, in animals, is important in its relation to disease in the human subject, but "its mechanism is difficult to explain" (p. 412).

Not a word does Mr. Paget tell us of the battle that raged so long and caused so much suffering to countless victims of the laboratory on this question of the glycogenic functions of the liver. To read the calm recital he gives us of the work of Bernard in this connexion

[1] *Op. cit.* p. 411.

it would appear that when Bernard had spoken the cause was ended. On the contrary, it raised a storm of protest amongst the physiologists, and when protests are made in that quarter the animal world must pay the penalty. M. L. Figuier, by a fresh research, convicted Bernard of error—

> " In demonstrating by an analysis more minute and more exact the existence of sugar in the blood of the portal vein of animals fed exclusively on meat." [1]

M. Metzger asks—

> " Who was to decide between these two champions, the one partisan, the other adversary of the glycogenic function of the liver? The suit was submitted to the Academy of Sciences, who charged a Commission chosen from its body to study and elucidate the problem."

Then began more experiments, more disputations. Space fails me to tell how Seegen, Abeler, Schiff, Rouget, Bock, Hoffmann, Roger, Chouppe, Pinet, Dastre, Bourguelot, Boehm, Kratschmer and many other experimenters had divers and conflicting theories about this pathology of the liver functions which Claude Bernard's great discovery was to have made so plain.

"Before Claude Bernard," says Mr. Paget, "the pathology of diabetes was almost worthless." And what is it to-day? The article on the pathology of diabetes in the *Text-Book of Medicine* by Drs. Fagge and Pye-Smith (vol. i. p. 454) says: "On the whole, we must admit that the riddle of glycosuria has not yet been solved," and—

[1] D. Metzger's *La Vivisection*, p. 112.

> "At present diabetes stands alone, its origin and nature undiscovered, and its relation to other diseases uncertain. We therefore place it by itself as a non-febrile general disease, with no ascertained pathology or anatomy" (p. 436).

And this after all that Claude Bernard, according to Sir Michael Foster and Mr. Stephen Paget, discovered concerning glycogen.

This is just the place to quote, once more, Bernard's hard-worked sentence, which to please Mr. Paget we will give in the original French: *Sans doute, nos mains sont vides aujourd'hui, mais notre bouche peut être pleine de légitimes promesses pour l'avenir.*

Should a too inquisitive lay reader ask, "What influence has all this research had upon practical medicine?" I should reply, "Little, if any at all." One of the most valuable text-books of medicine, that of Professor Felix von Niemeyer, says:—

> "The pathogeny of diabetes still remains obscure. The discovery of the physiologists, that sugar appears in the urine of animals after puncture of the floor of their fourth cerebral ventricle, has not yet thrown light upon the mystery. We know that the presence of sugar in the urine, whence diabetes mellitus derives its name, does not depend upon the functional abnormity of the kidneys; that the sugar is not formed in them, and that it is excreted from the blood; but we are altogether ignorant wherein the constitutional anomaly consists, in consequence of which a diabetic patient's blood contains sugar, and a healthy person's none. As the various hypotheses offered to account for diabetes are of little practical value, we shall

mention a few merely of those most generally entertained."

And if, greatly daring, my inquisitive friend were to ask, "What effect has this research had on the treatment of the disease?" I should reply, "Little, if any at all."

In the year 1779 Dr. Francis Hutchinson wrote to Dr. Cullen describing a case of diabetes under his care which yielded to treatment by opium amongst other things. Now opium to this day is the doctor's sheet anchor in one form or another for this malady. *Hooper's Medical Dictionary* (1839) recommended "confining the patient to an animal diet and enforcing an entire absence from every species of vegetable matter." At the present moment this is in the main the approved diet for diabetics. It has to a certain extent been modified by recent chemical research in food-stuffs, but animal food is to-day the main staff of a diabetic's diet. Notwithstanding all the important discoveries in physiology, interesting as they are in a scientific aspect, diabetes cannot be cured by them.

Were my friend presumptuous enough to push his questions still further he might demand to know "What effect has vivisectional experiment in diabetes had upon the death-rate?" I should be compelled with great regret to give him the reply of the Registrar-General:—

DEATH-RATE FROM DIABETES PER MILLION LIVING.

5 years 1861-65	5 years 1866-70	5 years 1871-75	5 years 1876-80	5 years 1881-85	5 years 1886-90	5 years 1891-95
29·2	31·8	35·8	40·4	51·4	62·4	69·4

But this does not appear in Mr. Paget's book.

V

THE PANCREAS

Mr. Paget's method in telling the story of discoveries in physiology is peculiar, and is well illustrated in this chapter on the pancreas. He begins by pointing out the absurd errors into which the old anatomists fell: "Anatomy could not see the things which belong to physiology" (p. 42). In his chapter on the blood he says: "They did not follow the way of experiment" (p. 4). "Physiology had been hindered for ages by fantastic imaginings" (p. 6), and so on. The one thing the old anatomists and physiologists needed was, in our author's opinion, experimentalism on living animals; they had living men and women (criminals) placed at their disposal by the State, but that does not seem to have helped them much.

In this chapter on the pancreas we are told how anatomy failed to help; then we learn how De Graaf discovered a way of collecting the pancreatic juice, and how Bohn experimented in 1710. Sir Michael Foster says that De Graaf's work was "very imperfect and fruitless." Why was this? De Graaf experimented on animals, but "physiology could not advance without organic chemistry" (p. 43). The pancreatic juice may be obtained by establishing a

D

fistula in the main pancreatic duct of a living dog (Bernard); but when the experiment is made,—

> "The secretion," says Flint,[1] "is readily modified by irritation and inflammation following the operation of making the fistula. The normal pancreatic juice is strongly alkaline, viscid and coagulable by heat. It is almost always the case that a few hours after the cannula is fixed in the duct the juice loses some of these characters and flows in abnormal quantity. With respect to susceptibility to irritation, the pancreas is peculiar; and its secretion is sometimes abnormal from the first moments of the experiment, especially if the operative procedure have been prolonged and difficult. That the properties above described are characteristic of the normal pancreatic secretion there can be no doubt; as in all instances fluid taken from the pancreatic duct of an animal suddenly killed in full digestion is strongly alkaline, viscid and coagulate by heat. This excessive sensitiveness of the pancreas rendered fruitless all the attempts to establish a permanent pancreatic fistula from which the normal juice could be collected (Bernard). The fluid collected from a permanent fistula does not represent the normal secretion."

If, as we are told, De Graaf's work was "very imperfect and fruitless," what reason have we to accept Mr. Paget's laudation of Claude Bernard's work, for we are told (p. 44) that he went back to De Graaf's method of the fistula? True, he had the

[1] *Human Physiology*, p. 245.

facts of modern chemistry to help him, and if Claude Bernard's discoveries were of any importance, it is quite certain, even on Mr. Paget's own showing, that they were due not to the experiments on animals, but to researches in physiological chemistry (p. 45).

Bernard held that the juice of the pancreas was the principal digestive fluid which acts upon starch. He was right, "although," says Flint, "he was in error in claiming that starch is digested almost exclusively by the pancreas." What led him astray? Let Professor Flint tell us: "Bernard's experiments, however, were made chiefly on dogs, and these animals do not naturally take starch as food."[1]

Mr. Paget tells us nothing of all this. Nor has he said a word about the contradictory experiments on animals made by Frerichs, Bidder and Schmidt, and many others, as to the physiological action of the intestinal juice in digestion,[2] which would have been very interesting. Busch made observations on the case of internal fistula in the human subject, which, says Flint, "have given the most satisfactory and definite information" concerning the action of the intestinal juice in digestion. The case was that of a woman who was injured in the abdomen by being tossed by a bull. The wound was below the umbilicus, and presented two contiguous openings connected with the intestinal canal.

> "It was supposed that the openings were into the upper third of the small intestine. At the time the patient first came under observation everything that was taken into the stomach was

[1] *Human Physiology*, p. 247.
[2] *Op. cit.* p. 242.

> discharged by the upper opening, and all attempts to establish a communication between the two by a surgical operation had failed.
>
> "At this time the patient was extremely emaciated, had a voracious appetite, and was evidently suffering from defective nutrition resulting from the constant discharge of alimentary matters from the fistula. Having been treated, however, by the introduction of cooked food into the opening connected with the lower end of the intestine, she soon improved in her nutrition, and was then made the subject of extended observations upon intestinal digestion."[1]

It is unnecessary, in this place, to detail the researches which were made on the digestion of different substances introduced into the wound of this woman's abdomen; suffice it to say that the results to science were only of less importance than were those of Dr. Beaumont on his patient, St. Martin, related in the chapter on the gastric juice. It has been from cases such as these, studied clinically, by competent observers and not by cruel experiments on dogs and cats, that we have acquired our knowledge of the digestive functions.

But Mr. Paget will have his vivisections in this connexion, and so he tells us how experimenters have removed the pancreas either wholly or in part from animals to study its relation to diabetes.

He tells us of the work of Dr. Vaughan Harley and others, and says that the facts they observed are as important as any that Bernard made out, "in no way contradicting his work, but added to it." This is

[1] Flint's *Physiology*, p. 242.

really a most remarkable statement to be made concerning physiological experimenters! Mr. Paget does not describe Dr. Harley's researches; some of them, if not all, were very cruel. In a paper contributed to the *Journal of Physiology* (vol. xviii. p. 1) Dr. Harley says:—

> "It is to be remembered that a dog, after the total extirpation of the pancreas, suffers from so severe a form of diabetes that it not only rapidly loses strength and weight, but that its entire metabolism must be very markedly disturbed. . . . I therefore, in the experiments about to be described, adopted the method . . . of keeping the dog fasting four days."

Starvation is part of the programme of the experiments. Minkowski says that in dogs complete removal of the pancreas is always followed by diabetes "if the animal lives long enough." The report of the experiment in the *British Medical Journal* (March 12, 1892) says:—

> "In a cat the author produced the same effect, but in rabbits he has not come to any conclusion, as complete removal is almost impossible. In a pig in which all but one-third of the gland was extirpated, sugar appeared five days after a meal of bread. It was diminished when meat was given, and disappeared after a day's fast."

Here was abundant experimentation on animals, but the account ends with the usual story of divided opinions amongst the experimenters. Thus—

"Lépine's view is that a ferment is produced by the pancreas which causes the destruction of the sugar, and that the absence of this ferment brings about diabetes. Minkowski says, however, that many more facts must be known before a clear explanation can be given."

And the experiments are still in progress. In the *Lancet* (Dec. 27, 1902, p. 1,753) there is a report of a meeting of the Pathological Society, at which Dr. E. H. Starling and Dr. W. M. Bayliss described some new experiments which they had performed on animals in connexion with the pancreas. We read that they introduced acid into the upper part of the small intestine to produce a flow of pancreatic juice :—

> "They then completely isolated a portion of the jejunum from all its connexions with nerves, and again introduced acid into the isolated gut, and a flow of pancreatic juice was again produced. They therefore thought it probable that the secretion of the pancreas was promoted, not by a reflex stimulation, but by a direct chemical effect through the blood. The next step therefore was to scrape the epithelium from the jejunum, pound it up with acid, and inject it into the veins of an animal; the result of such an injection was at once to produce a large flow of pancreatic juice."

If these experiments were performed under the influence of anæsthetics an abnormal condition would be produced, which would vitiate their results, and if they were performed without anæsthetics awful cruelty was involved. In the discussion which followed Dr. H. Batty Shaw said that one of the experimental suggestions "was not borne out by clinical experience."

Had Mr. Paget desired to add the weight of his testimony to the worthlessness of experimenting on animals as a means of advancing the art of medicine he could have chosen no more fitting subject than the account he has given of the cruel researches on the functions of the pancreas, nor could he at the same time have more effectually supported one of the chief contentions of the medical opponents of vivisection than by proving that experimental physiology is powerless to settle the difficult problems connected with the action of the fluids with which the food is brought in contact in the intestinal canal.

VI

THE GROWTH OF BONE

Mr. Paget has the remarkable habit of exalting the value of vivisection, by recalling the useless experiments of those who had made use of them up to the time when by some happy accident one of them had found out something of importance. Thus in telling us that "the work of Du Hamel proved that the periosteum is one chief agent in the growth of bone," he explains that "before him, this great fact of physiology was unknown; for the experiments made by Anthony de Heide (1684), who studied the production of callus in the bones of frogs, were wholly useless" (p. 47).

And what was Du Hamel's discovery? It had been found by John Belchier that the bones of animals fed near dye-works were stained with the dye. Du Hamel fed three pigs with ordinary food mixed with madder juice for a month, then stopped the juice, fed the animals in the ordinary way for six weeks, and then killed them. It was found that the marrow of the bones was surrounded by a layer of white bone. This was the bone formed before the madder was administered. Then this ring of white bone was surrounded by one of red bone; this was the formation of bone during the feeding with madder; finally, the red bone ring was covered by a layer of white bone; this was the ring formed after the madder had been discontinued.

Voilà tout. There was no vivisection, no torture, no cruelty, not even inconvenience to the animals in this perfectly legitimate series of experiments. But Mr. Paget had a cause to advocate and a book to write, and he could not afford to dispense with the least particle of favourable evidence. But though Du Hamel did something to explain bone-growth, he was far from revealing the whole of the secret, and his work was opposed by other investigators of whom Mr. Paget tells us, but he omits to say anything at all about the researches of Mr. Goodsir. Mr. Lawson Tait supplies the very serious omission in his answer to Mr. Sampson Gamgee's pamphlet, *The Influence of Vivisection on Human Surgery.* Mr. Tait says:—

> "Mr. Goodsir's conclusions are, on the contrary, uniformly accepted, and as to his method, he says that they were made upon shafts of human bones which had died—museum specimens, just as Du Hamel's were. They showed that whilst the periosteum is the matrix and machine by which the new bone is made, the real agency is in the layer of osteal cells, and so he finally solved the riddle. He did this by microscopic and pathological research. He condemned the employment of vivisection as useless and misleading, and to him we owe the completion of Belchier's and Du Hamel's research—a completion which was hindered for a century by the blunders of Vivisectionists."[1]

[1] *Uselessness of Vivisection,* by Lawson Tait, 1882, pp. 28, 30–33.

VII

THE NERVOUS SYSTEM

Mr. Paget deals with the researches of physiologists on the Nervous System in the same way as he dealt with the discovery of the circulation of the blood.

His object is to extol the value of experiments on animals, yet in these and other researches he records the numerous and grave errors into which experimenters have fallen for hundreds of years while following that mode of investigation. There was no restriction on the vivisection either of animals or of men from the time of the great Alexandrian physiologists through the period of Galen to that of the great Italian anatomists, yet Mr. Paget is fain to dilate on the mistakes of the ancient physiologists and the manner in which such discoveries as they did make became "obscured by fanciful notions of no practical value." The true path of physiological discovery—we are assured—is that of experiments on living animals. Herophilus of the Alexandrian School (about B.C. 335–280) is accused of having dissected alive as many as six hundred criminals. This is charged against him by Tertullian and Celsus as though it were a well-known fact. He was a great anatomist, and as the bodies of all malefactors were given over to the anatomists for the purpose of dissection, there was every opportunity for discovery, and Herophilus made many important additions to science; he knew the nerves of motion and sensation, but Mr. Paget omits to tell us about him, or that other great discoverer of the Alexandrian School, Erasistratus of Iulis (about B.C. 340–280), his contemporary, who also was familiar with the nerves of sensation and motion. The ancient apologists for the human vivisections of Herophilus and Erasistratus used to say that—

> "It is not to be called an act of cruelty, as some persons suppose it, to seek for the remedies of an immense number of innocent persons in the sufferings of a few criminals."

This is very much the same argument as Mr. Paget's friends use now with reference to the vivisection of animals, with the important distinction that it is the animal victims who are innocent.

Mr. Paget begins his history of the discoveries in relation to the nervous system with Galen, whose work, he tells us, was centuries ahead of his time, although he lived in the second century of our era, and the vivisectors of criminals had been at work four or five hundred years previously. Evidently in our author's opinion the human vivisections of the Alexandrian School counted for nothing. Galen was a great anatomist, he was also a vivisector, and made important discoveries concerning the nervous system, but physiologist as he was, it is certain that he owed much of his knowledge to the work of his predecessors. Mr. Paget says that the men who came after him let his facts be overwhelmed by fantastic doctrines! All through the ages from Galen to the Renaissance no great advance was made toward the interpretation of the nervous system. And at the Renaissance what happened? Mr. Paget again omits important matters. He does not tell us anything about the revival of human vivisection which came with the revival of learning. Surely if the vivisection of animals is of such great importance to human medicine, the vivisection of human beings would be still more valuable. Professor Andreozzi, in his book, *Leggi Penali degli Antichi e Cinesi*, has extracted from the Criminal Archives of Florence a number of cases in which criminals were handed over to the doctors to be dissected alive. The dates were from 1545 to 1570. Now Mr. Paget, overlooking these facts, says:—

> "This long neglect of the experimental method left such a gap (!) in the history of physiology, that Sir Charles Bell seems to take up the experimental study of the nervous system at the point where Galen had stopped short. . . . It is true that experiments had been made on the nervous system by many men; but a dead weight of theories kept down the whole subject."

We know that "dead weight of theories"; we are groaning under the microbe theories of disease to-day.

Mr. Paget asks:—

> "Why had men to wait so long for a better understanding of the nervous system? The one thing wanted was the experimental method; and, for want of it, the science of the nervous system stood still."

Experiments had been made, but they were not general, and "the unbiassed use of this method had been lost sight of." There seems to have been a singularly "unbiassed use" in Italy—the home of the great anatomists and physiologists—at all events.

In 1807 Sir Charles Bell published his great discovery of the functions of the nerves. We owe to him the knowledge that in the nervous trunks are special sensory filaments, whose office it is to convey impressions from the periphery to the sensorium, and special motor filaments which convey motor impressions from the brain or other nerve centre to the muscles. Bell dispelled the confusion previously existing amongst physiologists by his discovery of the functions of the nerves. He showed both from theoretical considerations and from the result of actual experiment on the living animal, that the anterior roots of the spinal nerves are *motor*, while the posterior are *sensory*. On November 26, 1807, he writes to his brother George:—

> "I have done a more interesting *nova anatomia cerebri humani* than it is possible to conceive. I lectured it yesterday, I prosecuted it last night till one o'clock, and I am sure it will be well received."[1]

One of our strongest proofs against the apologists of vivisection is the famous protest of Sir Charles Bell as to the misleading character of animal experimentalism. Sir Charles, in his book, *The Nervous System of the Human Body* (Longmans & Co., 1839, p. 217), says:—

> "In concluding these papers, I hope I may be permitted to offer a few words in favour of anatomy, as better adapted for discovery than experiment. Anatomy is already looked upon with prejudice

[1] See *Encyclopædia Britannica*, vol. iii. p. 541.

> by the thoughtless and ignorant; let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery, and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has done to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions. In a foreign review of my former papers the results have been considered as a further proof in favour of experiments. They are, on the contrary, deductions from anatomy, and I have had recourse to experiments, not to form my own opinions, but to impress them upon others. It must be my apology that my utmost efforts of persuasion were lost while I urged my statements on the grounds of anatomy alone."

This declaration has always been very discomforting to the apologists for vivisection, and it is natural that they should endeavour to discount its importance, and so Mr. Paget gives us several long quotations from Bell's works to prove that he was materially aided in his researches by his experiments, that the record of his vivisections appeared in his 1811 pamphlet while his work was still a new thing, and that it was not till his volume, which appeared in 1830 (when he was fifty-six, and the enthusiasm of youth had waned); that he was guilty of what Mr. Paget calls "inconsistency of sentences" which it is "impossible to reconcile." He tells us that Bell was by nature of a most complex and sensitive temperament, full of contrary forces—one man in 1811, another in 1830, and he tries very hard indeed to account for his remarkable attitude towards his former experiments. Mr. Paget, however, has unfortunately omitted to quote a passage from Bell's *Nervous System* (p. 31) which throws a powerful light on his method of research. He says :—

> "After delaying long on account of the unpleasant nature of the operation, I opened the spinal canal of a rabbit, and cut the posterior roots of the nerves of the lower extremity—the creature still crawled—but I was deterred from repeating the experiment by the protracted cruelty of the dissection. I reflected that the experiment would be satisfactory if done on an animal recently knocked down and insensible—that whilst I experimented on a living animal there might be a trembling or action excited in the muscles by touching a sensitive nerve, which motion it would be difficult to distinguish from that produced more immediately through the influence of the motor nerves."

Now if we refer again to the letter written by his brother in 1807 we find him exulting in his "new *anatomy* of the human brain." It was his anatomical work combined with his "theoretical considerations" which, like those of Harvey, led both to make their great discoveries, however much their vivisections aided them to demonstrate them. I do not deny that Bell vivisected, but I believe that we should have had the benefit of his great discovery had he never done so. Professor Flint—himself a pro-vivisector—in his great work *Human Physiology* (p. 549), says concerning the Nerve of Mastication :—

> "The anatomical distribution of the small root

> of the fifth nerve points at once to its uses. Charles Bell, whose ideas of the nerves were derived almost entirely from their anatomy, called it the nerve of mastication, in 1821, although he did not state that any experiments were made with regard to its action. All anatomical and physiological writers since that time have adopted this view. It would be difficult, if not impossible, to stimulate the root in the cranial cavity in a living animal; but its Faradization in animals just killed determines very marked movements in the lower jaw."

Mr. Paget says (p. 62): "The relation of Majendie's work on the nerve roots to Bell's work need not be considered here." But I must not omit it. Dr. Leffingwell says,[1] concerning experiments for demonstrating the functions of the spinal nerves:—

> "It was during a class demonstration of this kind by Majendie, before the introduction of ether, that the circumstance occurred which one hesitates to think possible in a person retaining a single spark of humanity or pity. 'I recall to mind,' says Dr. Latour, who was present at the time, 'a poor dog, the roots of whose vertebral nerves Majendie desired to lay bare, to demonstrate Bell's theory which he claimed as his own. The dog, mutilated and bleeding, twice escaped from under the implacable knife, and threw its front paws around Majendie's neck, licking as if to soften his murderer and ask for mercy! I confess I was unable to endure this heart-rending spectacle.'"

It was probably in reference to this experiment that

[1] "*Vivisection,*" *Lippincott's Magazine*, August, 1884, p. 129.

Sir Charles Bell, the greatest English physiologist of our century, writing to his brother in 1822, informs him that he hesitates to go on with his investigations. "You may think me silly," he adds, "but I cannot perfectly convince myself that I am authorized in nature or religion to do these cruelties."

Of Majendie we may say, "he had a devil."

> "It is droll," he says, "to see animals skip and jump about of their own accord, after you have taken out of their brains a little before the optic tubercles." And as to new-born kittens," he says, "they tumble over in all directions, and walk so nimbly, if you cut out their hemispheres, that it is quite astonishing."[1]

Mr. Paget refers very briefly to the experimental work of Flourens (1794–1867). In his euphemistic way he tells us that Flourens

> "Showed that the cerebellum (or lesser brain) is concerned with the equilibration of the body and with the co-ordination of muscular movements that an animal, a few days old, deprived of sensation and consciousness by removal of the cerebral hemispheres, was yet able to stand and move forward, but, when the cerebellum was removed, its muscles lost all co-ordinate action."

But this gives a very mild idea of the work which Flourens did in his laboratory in connexion with the functions of the cerebellum.

Let us take the following account of it from the *Hand-Book of Physiology*, by Dr. Kirkes, 8th ed., 1872. He tells us (p. 527) that

[1] *Journal de Physiologie*, t. iii. p. 155.

"Flourens (whose experiments have been abundantly confirmed by those of Bouillaud, Longet, and others) extirpated the cerebellum in birds in successive layers. Feebleness and want of harmony of the movements were the consequence of removing the superficial layers. When he reached the middle layers, the animals became restless without being convulsed; their movements were violent and irregular, but their sight and hearing were perfect. By the time that the last portion of the organ was cut away, the animals had entirely lost the powers of springing, flying, walking, standing, and preserving their equilibrium. When an animal in this state was laid upon its back, it could not recover its former posture; but it fluttered its wings and did not lie in a state of stupor; it saw the blow that threatened it, and endeavoured to avoid it. Volition, sensation, and memory, therefore, were not lost, but merely the faculty of combining the actions of the muscles; and the endeavours of the animal to maintain its balance were like those of a drunken man. The experiments afforded the same results when repeated on all classes of animals."

Physiologists tell us that the dangers attending this operation are so great that but few animals survive, and happily, for in the cases where they have lived for any considerable period the reports of these experiments have recorded the miseries which followed the operation; weakness of the legs, so that all the normal voluntary movements are interfered with, inflammations of the ears, eyes, joints and skin were amongst the consequences recorded by Bandelot.[1] The animal, we are

[1] *A Text-Book of Physiology*, Landois and Stirling, 3rd ed., p. 725.

told, died after eight months, after a period of wasting had set in.

As to the value of such experiments to practical medicine, Professor Carpenter says :—

> "I do not believe that on such subjects as the functions of the different parts of the encephalon, experiments can give trustworthy results; since violence to one part cannot be put in practice without functional disturbance to the rest."[1]

Marshall Hall and Reflex Action.

Before the time of Marshall Hall (1790-1857) reflex action, Mr. Paget tells us, had long been studied on decapitated vipers, frogs, eels and other animals, and he adds that "it cannot be said that these first studies of reflex action did much for physiology." As the animals experimented upon were decapitated the matter has no concern for us as anti-vivisectionists. In a decapitated animal, when the spinal cord is left intact, reflex phenomena altogether independent of sensation may be observed on stimulation of the sensory nerves. Marshall Hall discovered the definite centres in the spinal cord, and the experiments on beheaded animals being, of course, perfectly legitimate, we need not discuss them here, except to say that it was a long time before the experiments on the dead animals could be reconciled with the results of injury or disease of the spinal cord in human patients. But nothing in experimental physiology, it would seem, is ever finally settled. The *American Journal of Physiology*, vol. iii. No. 1, 1899, contains the records of some horribly cruel experiments upon the spinal cords of rabbits, cats, monkeys and dogs, and although we are told that the animals were anæsthetized,

[1] *Essay on Vivisection*, by J. Macaulay, M.D., p. 62.

we read on p. 49 that in the case of a cat subject to this mutilation, " towards the end of the experiment the animal commenced to gnaw its hind legs (showing the complete absence of sensation), and a considerable loss of blood was sustained in this way."

As the animal could bite it is evident that it was not decapitated, and it is much more probable that it was not properly anæsthetized, than that the gnawing of the legs proved that it did not feel pain.

Claude Bernard again

Mark Twain tells us, in his *Tramp Abroad*, that he heard so many works of art in Rome ascribed to the genius of Michael Angelo by his Italian guide, that when he was shown the Bay of Naples he was fain to exclaim: " By Michael Angelo I presume?" Mr. Paget has sounded the praises of Claude Bernard to such an extent that we should hardly have been surprised had we been told that the invention of the steam engine, wireless telegraphy and vaccination were really due to the prince of vivisectors. We begin with his experiments on p. 28 and are not finally rid of him till we reach the closing pages of the book.

Claude Bernard, we learn, discovered the vaso-motor nerves and the control of the nervous system over the calibre of the arteries. But in Dr. Munk's *Roll of the Royal College of Physicians*, vol. ii. p. 125, we are assured that Dr. Frank Nicholls (1699–1778), a famous anatomist and physiologist at Oxford, was one of the first to describe correctly the mode of the production of aneurism, and he distinctly recognized the existence and function of the vaso-motor nerves.

Then some of our best physiological writers, such as

Professor Flint, attribute the discovery of the vaso-motor nerves to Brown-Sequard. He says—[1]

> "This was the discovery of the vaso-motor nerves, and it belongs without question to Brown-Sequard, who published his observations in August, 1852. A few months later in the same year, Bernard made analogous experiments and presented the same explanation of the phenomena observed."

Actuated by that delicate reticence which distinguishes Mr. Paget's description of vivisectional experiments, we are spared any recital of the atrocious cruelties practised by Claude Bernard in his researches on the nervous system. Professor Flint, writing for students, is more outspoken; he says (p. 643):—

> "The vaso-motor fibres pass in the lateral columns of the cord, and from the cord, in the anterior roots of the spinal nerves, in the dog, as far down as the second pair of lumbar nerves. These fibres are medullated but are of small size. They pass to the blood-vessels either through branches from the sympathetic ganglia or through the ordinary cerebro-spinal nerves. They are therefore not confined to the branches of the sympathetic as Bernard has shown by the following experiment He divided the fourth, fifth, sixth, seventh and eighth pairs of lumbar nerves on one side in a dog, at the spinal column, and paralyzed motion and sensation in the leg of that side, but the temperature of the two sides remained the same. He afterwards exposed and divided the sciatic nerve in that side, and then noted decided increase in temperature. This ex-

[1] *Human Physiology*, p. 641.

> periment, which is only one of a large number, shows that the ordinary mixed nerves contain vaso-motor fibres, which are entirely independent of the nerves of motion and sensation, a fact which is now well-known by physicians and has frequently been illustrated in cases of disease in the human subject."

Interesting and important as is the study of the vaso-motor tracts it must not be imagined for a moment that Claude Bernard or Brown-Sequard settled the functions of that mechanism once and for all. Experiments in this direction are still pursued and are still unsatisfactory in their results. "Anæsthetics and curare affect more or less profoundly the irritability of the whole vaso-motor mechanism, and from animals under the influence of such drugs nearly all our data concerning the normal working of the vaso-motor apparatus have been gathered." [1]

It follows from this that the experimental researches, involving countless experiments on animals, have been worthless. Each form of investigation has in its turn proved futile. Anæsthetics and curare interfere with the results, and if the animals are experimented upon without drugs the pain would affect the blood-pressure, for Dittmar has shown that the feeblest stimulation of the sensory nerves, even when so slight as not to be felt by the animals, caused elevation of blood-pressure.

Cerebral Localization

The history of the mapping out of the functions of the brain occupies Mr. Paget's concluding section on

[1] See a paper on "Plethysmographic Studies," by H. Sewel and E. Sandford, in the *Journal of Physiology* (March, 1890, p. 185).

the Nervous System. It would be an intensely interesting story if it were not for the long list of cruelties which stain the record at almost every point. But of course our author tells us nothing about these. The cruelties are the omissions, the supposed results alone are recorded by our opponents. If we turn to Kirke's *Hand-Book of Physiology* (8th ed., published in 1872, p. 536), we shall find a very fair account of the state of what is termed localization of the functions of the brain at that time.

After intimating that "it is possible that each faculty has a special portion of the brain appropriated to it as its proper organ," and describing the work of Gall and Spurzheim in relation to their theory of phrenology, the author says :—

> "That this is a system of error there need be no doubt, but it is possibly founded on a true theory; the cerebrum may have many organs, and the mind as many faculties, but what are the faculties that require separate organs, and where these organs are situate, are subjects of which only the most general and rudimentary knowledge has been yet attained."

It was at this time that I had the good fortune to be a pupil of Dr. Hughlings-Jackson, of whose work Mr. Paget's book tells us (p. 74) that

> "The first step of the new discovery was constituted by the clinical and pathological observations of Hughlings-Jackson, which suggested the existence, on each side of the fissure of Rolando, of special centres for the movements of the leg, arm and face."

This was written by Sir William Gowers fourteen years ago. Although I was an earnest student of Dr. Jackson's I cannot remember that I ever heard him mention experiments on animals; our work, under his direction, was to observe as closely as possible the symptoms and physical signs exhibited by patients in the hospital wards who suffered from any form of nerve or brain disease, and having carefully noted them in our case-books, to avail ourselves, when the patients died, of any opportunity that was offered us in the *post-mortem* room of correcting our diagnosis by anatomical work on the bodies of the sufferers who had succumbed to their maladies.

I well remember, in cases where the patients suffered from interference with the faculty of speech, how we searched in the left hemisphere of the brain at the *post-mortem* examination, and particularly in that region thereof known as "Broca's convolution."

All this portion of Mr. Paget's book is well and fairly enough stated. He tells us (p. 72) that

> "clinical observation and *post-mortem* examination found the speech-centres, physiological experiments had nothing to do with it."

For practical purposes it may be said that Gall, towards the end of the eighteenth century, discovered that different areas of the brain are connected with different mental and physical manifestations.[1]

The clinical and anatomical work of Hughlings-Jackson led to the experiments of Ferrier on living animals, but the German experimenters, Hitzig and

[1] See *The Wonderful Century*, by Alfred Russell Wallace, F.R.S., pp. 160, *et seq.*

Fritsch, had been working on the same lines previously, and numerous researches by other vivisectors led to such opposing conclusions that the *Lancet* (November 10, 1883, p. 823) said:—

> "It must be confessed that the aid localization has afforded to treatment has been small and practically confined to cases of surgical interference. Even of these cases there are very few—scarcely more than could be counted on the fingers of the hand—in which the power of localizing cerebral disease can be said to have been the means of saving a life that would have been lost without it."

Professor Charcot points out, in his *Leçons sur les Localisations dans les Maladies Cérébrales*,

> "That the utmost that can be learned from experiment on the brains of animals is the topography of the *animal* brain, and that it must still remain for the science of human anatomy and clinical investigation to enlighten us in regard to the far more complex and highly differentiated nervous organization of our own species. And, in fact, it is in the department of clinical and *post-mortem* study that, so far, all our best data for brain localization have been secured."[1]

Dr. Hermann—Professor of Physiology and Medical Physics, Zurich University—says of the experiments of Fritsch and Hitzig:—

> "However interesting and precious they may

[1] Dr. A. Kingsford, *Illustrated Science Monthly*, February, 1884.

be, do not justify any conclusions concerning the functions of the cortex." [1]

"Physiological experiments," says the Professor in another place, "conducted in these regions are most indefinite." [2]

Sir William MacCormac, delivering the Hunterian Oration at the Royal College of Surgeons on February 14, 1899, said:—

> "I doubt if our modern surgery, despite its knowledge of cerebral localization, can advance much further than Hunter was prepared to go."

John Hunter died in 1793!

Mr. Paget says (p. 77):—

> "There have been, now and again, differences of interpretation of this or that fact, diversities of results, and problems too hard to solve, and other difficulties, such as befall all the natural sciences; but these imperfections amount to very little, when the whole result comes to be reckoned up. The marvel is that the work is so nearly perfect, seeing its immeasurable complexity."

Mr. Paget is a polite man and in these remarks has sublimated his euphemism, for never in the history of medicine can harder words have been more freely bandied than were employed by rival physiologists concerning the results of their experimentation on the brains of animals in this connexion. Mr. Paget gives us a quotation from Sir Michael Foster; let me

[1] *Pfluger's Archiv.*, vol. x. pp. 78–84.

[2] *Hermann's Human Physiology*, translated by Prof. Gamgee (London, 1878), p. 444.

also quote that distinguished physiologist in relation to rival experimenters. In a speech delivered before the Pharmaceutical Society, reported in the *Pharmaceutical Journal* (May 25, 1895), he said that—

> "The very spirit of a scientific man was to believe that his brother was a liar, and that his one duty was to prove it."

The researchers in the localization of brain functions have very consistently acted in this spirit, at every point the physiologists quarrelled vigorously amongst themselves as to the true interpretation of their experiments. Dr. E. S. Reynolds, in an article in the *British Medical Journal*, February 11, 1899, on "The Uncertainties of Diagnosing Brain Tumour," said :—

> "It will be seen from the above sketch of a very large subject how difficult it may be for a neurologist to give an accurate opinion on the precise locality and nature of a brain tumour when required to do so by a surgeon before he undertakes an operation; and it can be understood that the difficulties and uncertainties of brain surgery are not so much those of surgical technique or of untoward after effects, but occur at the outset, and with our present knowledge may be well nigh insurmountable."

Whatever may be the mere scientific value of these experiments, so far as practical surgery is concerned, Messrs. Rose and Carless, in their well known work, *A Manual of Surgery*, say (p. 513) :—

> "For practical purposes the above measurements suffice for a foundation to work out a

complete topography of the brain; and after all, when it is a matter of operation, the surgeon does not usually limit his field to a single trephine aperture."

In the *Polyclinic* for February, 1903, there is an article by Dr. James Taylor on "The Question of Operation in Epilepsy." He says with reference to such a case on which it was proposed to operate:—

> "As a result of general experience it is found, that except in cases where there is a depression of the bone, operation in cases of traumatic epilepsy is not a hopeful procedure. The surgical wounding of the brain tissue is necessarily followed by the formation of scar tissue, and this continues the irritation of the already weakened brain cells. In view of these facts and experience, it was decided that the present case was unsuitable for operation."

Of course, where there is a depression of bone no theories of localization of function are involved. The local injury guides the operator's hand.

PART II

EXPERIMENTS IN PATHOLOGY, MATERIA MEDICA AND THERAPEUTICS

I

INFLAMMATION, SUPPURATION AND BLOOD POISONING

The *British Medical Journal* said (Jan. 9, 1875) that "without vivisection experiments we would know almost nothing of the phenomenon of inflammation." Mr. Paget, by implication, would lead his readers to form a similar ridiculous idea. Now let us turn to a standard text-book of up-to-date Surgery. Take, for example, the *Manual of Surgery*, by Messrs. Rose and Carless. On page 18 of that work we read:—

> "The actual phenomena of inflammation may perhaps best be studied in the web of a frog's foot. If this be spread out and examined under the microscope, the following evidences of physiological activity may be seen," etc., etc.

Now this simple observation entails no pain whatever to the animal, who merely suffers the inconvenience of being restrained in a possibly rather uncomfortable position. The authors, after describing the normal appearances of the circulation of the blood in the transparent web of the frog's foot, go on to say:—

> "If now a crystal of common salt, or some such irritant, is applied to the web, the early vascular

phenomena contributing to inflammation may be readily observed."

Mr. Paget ridicules the classical definition of inflammation as "redness and swelling with heat and pain," which sufficed for generations of physicians and surgeons, and tells us that we must look to the vivisectors for a more scientific description. As this definition very exactly describes the state of affairs that obtains in cases of inflammation, it is good enough for the practical purposes of the surgeon, notwithstanding the additional light cast on the subject by the experimenters on animals. Diapedesis, or the passage of the blood-corpuscles through the walls of the vessels, may be observed in the frog, as already explained, but Professor Flint says [1] :—

> "It is not certain that diapedcsis, even of leucocytes, is a normal process, or that it takes place in the human subject. According to Hering, the red corpuscles pass through the walls of the vessels only when the pressure is sufficient to produce transudation of the blood-plasma."

That many experiments upon animals have been made in this connexion by no means implies that inflammation can only be properly understood by resorting to them.

Very candidly does Mr. Paget tell the story of Semmelweis, who, long before Lord Lister's researches on the putrefaction of wounds, discovered antiseptic surgery.

The tragic end of Semmelweis, who by insisting on hygienic measures in the lying-in wards of the general

[1] *Human Physiology*, p. 104.

hospital at Vienna reduced the mortality, which in May, 1847, had reached 12·24 per cent, to 1·27 in 1848, is one of the saddest in medical history. His mind gave way in consequence of the persecution of his professional brethren, and he died in a lunatic asylum; and Mr. Paget tells us that "his work was lost just for want of experiments on animals" (p. 90).

The facts are very simple. Semmelweis saved his patients, and banished puerperal fever from the maternity wards by perfecting a system of absolute cleanliness, which he insisted on being carried out in his hospital; but because he did not vivisect, his jealous, bigoted, ignorant fellow-surgeons would not believe him, and refused to follow his teaching, as they could not comprehend that by cleanliness Nature works miracles of healing.

The antiseptic method needed no experiments upon animals. Hippocrates came very near it when he made use of "raw tar-water" in the treatment of wounds. Mr. Watson Cheyne has proved that germs can flourish in wounds that are perfectly aseptic, and live very well indeed in solutions of chromic acid, and further, that abscesses which have never been exposed to the air may be crowded with germs and living organisms.[1]

Mr. Paget admits that "it has indeed been shown that suppuration may in exceptional conditions occur without micro-organisms" (p. 96). This is sufficient for my purpose; a chain is no stronger than its weakest link.

[1] *Transactions of the International Medical Congress*, 1881, vol. i. p. 321.

II

ANTHRAX

Mr. Paget devotes several pages of his book to an account of the inoculation of animals against anthrax, charbon or splenic fever; but he is not enthusiastic about it, and concerning the antitoxin for the treatment of men accidentally inoculated by the disease (which is then called malignant pustule) he says, "for its treatment in man, an antitoxin has been used with some success; but the cases are too few to be of importance" (p. 101). Pasteur's vaccinations against anthrax have been largely employed in France and Italy, but apart from the interested reports of their value issued by the Pasteur Institute in Paris, the accounts of experts as to their value as a preventive measure are not by any means encouraging. As to the treatment of human beings subjects of either malignant pustule or wool-sorters' disease, the surgical practice is thus described in *The Manual of Surgery*, by Rose and Carless (p. 128):—

> "The treatment must be active and energetic where possible. In the local affection, free excision of the necrotic patch and of all the infiltrated tissue around, and the application of either the actual cautery or of pure carbolic acid

is the only hope. For the general disease, merely symptomatic treatment can be adopted."

Whatever may be thought of protective inoculations of animals in France against anthrax, they are not favourably regarded in this and other countries. Our Board of Agriculture has issued the following

LEAFLET ON ANTHRAX A $\frac{1-95}{1}$

"Inoculation on the system recommended by M. Pasteur could not be adopted except by an expert accustomed to operate; but the results of the operations in this country and elsewhere have not been of such a nature as would warrant the Board in recommending it to stockowners as a means of dealing with outbreaks of anthrax."

In Hungary the Government was recommended by its Commission to prohibit the use of M. Pasteur's *Vaccines*; and they met with no better treatment in Germany. Dr. Koch concludes that natural infection is different from, and more fatal than, infection conveyed by inoculation, and that the protective inoculation with M. Pasteur's *Vaccines* is of little avail against *natural* infection.[1]

Professor McFadyean, of the Royal Veterinary College, London, addressing the annual meeting of the Western Counties' Veterinary Association, held at Exeter, is reported in the *Western Morning News* of March 25, 1898, as having thus expressed himself on these inoculations. He said:—

[1] From *L'Inoculation Preventive du Charbon. Reply to M. Pasteur.* By Dr. Robert Koch (p. 30).

"Despite the success which had been obtained in France, the system had not found universal favour in this country, where, he admitted, it had had a restricted trial. He gave his own experience of the system on sheep, which he admitted resulted in a large percentage of fatal cases, and he did not advise any British veterinary surgeon to press this method of treatment on a reluctant stockowner."

III

TUBERCLE

Mr. Paget begins his remarks on Tubercle by telling us that Laennec's invention of the stethoscope and his great discovery of the specific nature of tubercle "place him almost level with Harvey." "He founded the facts of tubercle." But Laennec's invention of the stethoscope had nothing whatever to do with experiments on animals. His g eat discovery was purely accidental—a fact which he declares in his famous work.

> "In 1816 I was consulted by a young woman labouring under general symptoms of diseased heart, and in whose case percussion and the application of the hand were of little avail on account of the great degree of fatness. I happened to recollect a simple and well-known fact in acoustics, and fancied it might be turned to some use on the present occasion. The fact I allude to is the great distinctness with which we hear the scratch of a pin at one end of a piece of wood, on applying our ear to the other. Immediately, on this suggestion, I rolled a piece of paper into a kind of cylinder, and applied one end of it to the region of the heart and the other to my ear, and was not a

little surprised and pleased to find that I eould thereby pereeive the aetion of the heart in a manner mueh more elear and distinet than I had ever been able to do by the immediate application of the ear." [1]

If this is an example of what Mr. Paget means by "experiments on animals" in relation to the diseovery of the stethoseope, he entertains peculiar notions on his subjeet.

We are told (p. 111) that although physicians had long known that phthisis was, or might be, infective,

> "they made nothing of it; they waited three centuries for Villemin to inoeulate the rabbits, and then the thing was done."

What was done? Was the remedy diseovered? Not at all; there was—

> "A short period of uneertainty; different species of animals are so widely different in their suseeptibility of the disease that the results of further inoeulations seemed to go against Villemin."

Yet even to-day experiments on animals proceed very mueh on the lines of the railway porter in *Punch*, who said, "Cats is dogs, and rabbits is dogs, and so's parrots; but this 'ere tortoise is a inseet, so there ain't no eharge." Professor Koeh acted on this prineiple when he transferred the results of his laboratory experiments with tubereulin from guinea-pigs to human beings with the terribly fatal results that are so well known to all. He says, in his paper, in a famous passage which Mr. Paget has not found it neeessary to quote—

[1] Laennec, *Treatise on Diseases of the Chest*, p. 5.

> "It was soon shown that the action of the remedy on man differed in some important respects from its action on the guinea-pig, the former being far more susceptible to it than the latter. Thus, a healthy guinea-pig may have as much as 2 c.c. injected subcutaneously without being notably affected by it; but in a healthy adult man as little as 0·25 c.c. suffices to excite intense reaction. In other words, regarding the relative body weight, $\frac{1}{1500}$ of the quantity which has no appreciable effect on the guinea-pig is most powerfully active in man."

All the world knows what this laboratory experimentation led to. Professor Koch carried the conclusions drawn from his experiments on these guinea-pigs to the anxious patients who had flocked to Berlin in consequence of the allurements he held out by his "great discovery"; but the poor men and women were not constituted like the guinea-pig, and as Mr. Paget says (p. 112), "Its failure was one of the world's tragedies."

An article appeared in *Blackwood's Magazine* (Jan., 1895) which paints the awful picture of Koch's *debâcle* in its true colours.

> "He raised a frantic hope in the bosoms of sufferers all over the world, and of those who loved them. He caused such excitement as has rarely been known before, both among the learned and the ignorant. And then it all came to nothing. The disappointment, the disillusion, was immense; and as a matter of fact, from standing out against the sky as one of the great benefactors of the human race—as he did for some time, with all sorts

of substantial rewards and honours prematurely bestowed—the figure of Koch has disappeared altogether, to be hailed by nothing but a grin or a groan, according to the disposition of the spectator, should it make any future appearance again."

That future appearance took place at the meeting of the British Congress on Tuberculosis held in London in July, 1901. By many experiments on animals it had been proved to the satisfaction of the whole bacteriological world that the bacillus of tubercle was disseminated largely by the milk and flesh of tuberculous animals. The scientific world bowed to this authority, and chased the bacillus through the byres and dairies of the planet, and now at this Congress Koch appeared once more, laughed in our too credulous faces, and told us that all our pains with food and flesh have been thrown away. By more researches, more laboratory experiments on animals, he has proved to his own satisfaction that the tuberculosis of animals is not transmissible to man.

Virchow and all the other bacteriological experimenters were against him, and so they set to work to prove by yet more experiments that Koch is wrong once again. Mr. Paget says (p. 113) that the discovery of the tubercle-bacillus "has greatly helped to bring about the present rigorous control of the meat and milk trades." Dr. Koch told the world that such control is quite unnecessary.

That tuberculin might not be wholly wasted and its discovery not a complete fiasco, it was next introduced as test for the detection of incipient tuberculosis in cattle. "The injection of tuberculin is followed in eight to twelve hours by a well-marked rise of temperature, if the animal be tuberculous." At first sight this seems to be a valuable aid to diagnosis. But the *Farmers' Gazette*, January 2, 1897, says that "tuberculin is not an absolutely perfect test." Prof. Brown, in his report of the Veterinary Department of the Board of Agriculture, says:—

> "It has been proved that one effect of inoculating animals with tuberculin has been in some cases that of distributing the tubercle-bacillus to parts of the body not previously affected."

By repeated injections it is alleged that tuberculous cattle may be sold as healthy, as they cease to react to the test, and their milk and flesh may thus be disseminated broadcast on the strength of this delusion.

Koch's discovery of the micro-parasite of tuberculosis has been utterly unproductive of the smallest improvement in the treatment of consumption. *Non tali auxilio.*

Travellers tell us of strange people who invoke the aid of mountain peaks for the cure of their diseases. They may do so with greater certainty of relief than seeking assistance from the bacteriological laboratories. The fresh air cure, especially the air of mountains, is the only treatment that has been proved of any real service in dealing with the direst disease prevalent in this country, and no animal experimentation taught our doctors this.

IV

DIPHTHERIA ANTITOXIN

Mr. Paget devotes thirty-two pages of his book to the praise of diphtheria antitoxin. He gives us columns of statistics and quotes many authorities in its favour. We need dispute neither. The treatment of diphtheria by antitoxin is now the mode. Medicine, like everything else, has its fashions, which must be conformed to whilst the craze lasts. It is said that

> "When antitoxin is given, on the first day the mortality is as low as 2 per cent., and on the second day as low as 6 per cent.; while, if it is given as late as the third day, the mortality is 30 per cent.; and when as late as the fourth day, 50 per cent."

But these figures only mean that immediate and active treatment in such a disease as diphtheria is of the utmost importance. Skilled treatment and nursing would show as good results as these if antitoxin were unknown, could the cases come under treatment the first or second day. It must be observed that even when antitoxin is employed, the local and medicinal treatment is carried out as formerly. Physicians when called to a case of diphtheria do not rely solely on antitoxin. Antiseptics are used locally, and tincture of perchloride of iron is most usually given as a medicine. Notwithstanding the use of diphtheria antitoxin, the death rate from the disease is steadily on the increase. Probably this may after all be largely a matter of diagnosis and nomenclature. Many throat diseases are now classed as diphtheritic which were not formerly so regarded.

Dr. George Wilson, in his well-known work *A Handbook of Hygiene and Sanitary Science*, 8th ed., pp. 436–7, attributes the alleged success of the antitoxin treatment of diphtheria to the following causes: (1) The fact that the disease, like scarlatina, is becoming of a much milder type; (2) That patients subjected to the antitoxin treatment are watched and nursed more carefully; (3) The mere injection of the antitoxin in all but the very young acts as a "faith cure"; and (4) Local and constitutional remedies are not discarded, and are doubtless used by the doctor

> "with a keener discrimination and more timely application, instead of yielding to an attitude of despair as he formerly did, because he had no sheet-anchor on which he believed he could rely."

He adds that if antitoxin has, as is alleged, so potent a remedial agency, the reduction of the mortality ought to be much greater than it has turned out to be after such extended and protracted trial. The chief successes of the treatment have been achieved in the hospitals, and there is little doubt that the far greater attention now paid to nursing and the local treatment of these cases has had much more to do with the recoveries than the serum injected. Professor Sims Woodhead said in a lecture delivered at the Royal Institution in 1895:—

"It should not be accepted that this agent can reduce the cure of diphtheria to a mere process of injection. Everything must be done to improve the conditions under which the patients are treated, to maintain their strength, to give them fresh air, cleanly surroundings, and good general hygienic conditions. It will be found withal that a certain number of deaths from rapid poisoning will take place, while a number of others will succumb in the later stages of the disease. This serum can no more act as a specific in every case than can quinine cure every case of malaria. . . . The antitoxic serum treatment is only one of our lines of defence against this disease."

V

TETANUS

Very little can be substantiated in favour of the treatment of Tetanus by antitoxin. Messrs. Rose and Carless [1] say that

> "At present the results of this treatment have proved disappointing, since few cases of acute tetanus have been cured by it, and the effect even in the more chronic cases is not at all certain."

Dr. Whitla, in his *Dictionary of Treatment*, says (p. 938) :—

> "Notwithstanding all the reported successes, there is little evidence of the value of injections of the antitetanic serum. Great numbers of cures are recorded in those cases where the period of incubation was prolonged much beyond the average noticed in severe cases, and it must be remembered that these are the class of cases where a spontaneous cure is the most likely to occur, and many reported successes would doubtless have been safe without the serum."

So dissatisfied were surgeons with the hypodermic

[1] *A Manual of Surgery*, p. 124.

use of this remedy that it has been injected into the brain. The Paris correspondent of the *Lancet* reported (March 25, 1899, p. 870) that at the meeting of the Society of Surgery, M. Quénn read the notes of five cases of tetanus treated by injecting antitoxin into the brain. Every one of the patients so treated died. M. Chaput and M. Ricard have each reported a case of death after these brain injections. M. Championnière said that no treatment could be definitely recommended. The curative value of the use of the serum was still far to seek.

VI

RABIES

The Pasteur treatment in relation to hydrophobia rests on a very different basis from that of any other disease which we have been considering. The Pasteurians will have nothing to say to a patient actually suffering from the malady, but if he has not contracted it, but is only afraid that he may have it incubating in his system, then at the Pasteur Institute he can be treated, and, as is alleged, the accession of the dread disease can be prevented. In other words, the treatment is prophylactic and in no way curative. It will readily be understood how difficult, how impossible indeed, it is to deal with statistics, however diligently compiled and analyzed, in a matter of this kind. I have before me a very carefully compiled list of 1,857 reported deaths of persons who have actually died of hydrophobia after undergoing the Pasteurian preventive inoculations at one or other of the recognized Institutes established for that purpose. The list lengthens day by day. It has been alleged, and not without foundation, that Pasteur did not cure hydrophobia, but gave it. Certain it is that what was called his "intensive" or "rapid" method of inoculation was liable of itself to produce hydrophobia; but apart from that, the regular inoculations that I have witnessed at the Institute in Paris appeared to me a very filthy and dangerous method of dealing with the human organism.

> " The first step in Pasteur's method is to obtain a definite, strong virus which will always produce death in a given fixed time when injected into an animal. This powerful virus is only obtained after many inoculations, and, when procured, its lethal action is singularly uniform. It is developed in the following manner :—A rabbit is inoculated under the *dura mater* with the virus from a rabid dog, and an emulsion from the medulla of the victimised rabbit is injected into another rabbit, whose medulla is in turn used for the inoculation of a third, and so on. After each inoculation the resulting virus becomes stronger, and the incubation period shorter, till, as the virus gains in virulence, the period of incubation becomes fixed at six to seven days.
>
> " The virus of the dog must be made to pass through a hundred rabbits before this uniform or fixed virus is obtained. It is then many times more powerful than the ordinary virus of a rabid dog, and stronger than the virus from a rabid wolf.
>
> " A rabbit being inoculated with this fixed virus, takes ill upon the sixth day, and dies upon the tenth day after inoculation. If the spinal cord of this animal is now removed and exposed in a sterilised jar or bottle to air deprived of moisture, by the presence of caustic potash, and kept at a temperature of 77° F., it is found that every day produces a diminution in the power of its contained virus. An emulsion, made by rubbing up a por-

tion of the cord before drying, causes rabies to appear in an inoculated animal in six days and death supervenes in ten days, as first stated. After drying the cord for eight days, and inoculating with it, the animal so treated does not die till about the twenty-fifth day. After drying the cord for fourteen days, no effects whatever follow its inoculation.

"Pasteur's method of treating patients bitten upon the limbs or trunk by rabid animals is, as soon as they present themselves upon the first day, to inject an emulsion of a spinal cord, which has been dried for fourteen days and also an emulsion of a cord dried for thirteen days. The emulsion is prepared by crushing about half a cubic centimetre (=the volume of about seven and a half minims of water) of the rabbit's cord in about thirty minims of sterilized beef tea, and the injection is made under the skin of the abdomen."[1]

On the third day the inoculation is from a cord dried twelve days, and so on. To prepare the inoculations, therefore, it is necessary to submit a vast number of animals to very cruel and prolonged suffering. No one can visit the great menagerie of animal victims attached to the Paris Institute without being struck by this fact, but my business here is with the results of the experiments, and not alone with the cruelty involved.

Statistics in this connexion are, and must always be, of an entirely worthless character.

"The difficulties in the way of a sound conclu-

[1] Whitla's *Dictionary of Treatment*, pp. 431-32.

sion are great. For, first, no one knows how many of the people who have been inoculated had really been bitten by a rabid dog at all. Secondly, when a bite is inflicted on a part covered by clothing, the venomous saliva is generally wiped off, and so the person bitten escapes. Thirdly, many who were bitten had been well treated by caustics, and thus the virus may have been, and no doubt frequently was, destroyed before they were inoculated. Lastly, we do not know the proportion of men or animals who, from some 'insusceptibility' or idiosyncrasy (i.e. some individual unknown cause) escape the disease even when the virus is fairly injected, though, judging from analogy from syphilis and from vaccinia, it is probably small.

"Another question has also to be considered, whether the intended protective inoculation may not, if unwittingly employed on persons who have not really been infected before, produce a fatal form of the very disease which it is supposed to protect. It cannot be said that these questions are fully answered, or that all the difficulties have been surmounted." [1]

[1] *A Text-Book of Medicine*, Fagge and Pye Smith, vol. i. p. 413.

VII

CHOLERA

One of the most amazing things in Mr. Paget's special pleading in favour of vivisection is the inclusion of cholera amongst the subjects of experimentation on animals. Animals are not liable to the disease at all. Cholera is believed by bacteriologists to be caused by the invasion of a vibrio called incorrectly "the comma bacillus." That this organism is found in the intestinal contents of cholera patients is perfectly true, but good authorities hold that "several microbes may cause cholera as several microbes cause suppuration." Be this as it may, experiments on animals have not helped the treatment of cholera in any way. Drs. Fagge and Pye Smith, in their great *Text-Book of Medicine*, say (vol. i. p. 284) :—

> "Koch's inoculations were far from convincing, and the attempts of others to reproduce cholera by introduction of the commas into the stomach or intestine or blood of guinea-pigs and other animals have not succeeded, even when the acid digestion of the stomach has been evaded, or when the influence of the bile has been excluded by ligature of the bile-duct, or peristalsis stopped by opium. Whether man is the only animal capable of contracting

> cholera, or whether the right animal has not been found—for the earlier experiments of Thiersch and Sanderson above mentioned are inconclusive—are questions not yet decided."

Mr. Paget has filled the pages of his chapter on cholera with the records of the work done and the investigation of the disease in India by Haffkine and others. But these experimenters made their researches upon themselves, not upon animals. Some of these experiments were "often dangerous and always laborious," as he observes, but what they had to do with the subject of Mr. Paget's book, except to pad it out, passes my comprehension. We are all ready to accord our word of praise to the self-sacrificing efforts of medical scientists in their work of curing disease, but it is the torture of animals we opponents of vivisection condemn, not the legitimate work of the physician.

VIII

PLAGUE

Mr. Paget tells us that the first experiments in preventive inoculation in animals were made by Yersin and others in 1895. Gliding adroitly over very thin ice our author tells us nothing about the success attending Yersin's inoculations of human beings. The *Medical Week*, September 3, 1897, said, in reference to the laudatory reports on the subject by certain experimenters :—

> "The curious part of it is that this beneficial action has passed unnoticed on the spot. Our correspondent at Bombay, who is chief medical officer to one of the hospitals at that place, writing under date of July 30 last, says : 'Nothing here is said of Dr. Yersin, whose treatment, I find, is entirely discredited here by the medical profession and laymen alike.'"

Mr. Paget treats us to no less than twenty-nine pages, devoted to the laudation of M. Haffkine's inoculations against plague in India. The relation of his statistics and encomiums to the broad facts about the success of the plague-serum is very similar to that of Falstaff's bread to his wine. "Oh, monstrous! but one halfpennyworth of bread to this intolerable deal of sack."

In reading the chapter on Plague Inoculations the uninitiated would be led to believe that their virtues were acknowledged by experts everywhere, and only "lying antivivisectionists [1] and political agitators" found any with them. Let us see what the medical authorities have to say on the matter. The *Medical Press*, March 14, 1900, in an article on the Anti-Plague Inoculation Commission, said :—

> "The broad result is encouraging for the future of this particular branch of serum-therapy, although its failure to establish a series of definite scientific conclusions must come as a disappointment to enthusiasts of preventive medicine. The difficulties of this task have been simply enormous, and the Report practically amounts to an admission on the part of Mr. Haffkine that he has failed to obtain sufficient data to place his prophylactic serum among the established truths of medicine."

The following important criticism of the inoculations appeared in *The Hospital*, March 3, 1900 :—

> "The report of the Indian Plague Commission, so far as concerns the question of anti-plague inoculation, has now been given to the world. The report begins with a general historical survey of the subject of preventive inoculation, a survey which is sufficient to show on how broad a basis this proceeding now stands. Nevertheless, it is clear that much yet remains to be done before we can say with anything like assurance that we have in Haffkine's inocula-

[1] Mr. Paget's polite phrase is "all sorts of lies are told about it, partly by anti-vivisectionist writers, partly by native political agitators, partly by the Hakims."

tion a satisfactory and reliable prophylactic against plague. The Commissioners criticize somewhat severely the methods adopted in the preparation of the vaccine, especially in regard to their uncertainty in producing an aseptic product, and they show that, as a fact, a number of the samples tested gave evidence of contamination of the prophylactic. Then they are very severe upon the methods of standardisation which seem to have been employed, and when one reads the details one can hardly say that they are too severe in their condemnation. It had been understood that the vaccine as sent out had been so standardized that a given volume would, on the average, produce a given result, but the Commissioners elicited that 'the routine practice which was adopted in Mr. Haffkine's laboratory was to standardise the vaccine by holding up to the light one or two sample bottles of each brew with a view to appreciating the opacity of the vaccinating fluid. It was in conformity with the results of this appreciation that the dose was inscribed on the label of each bottle. This standard dose was an arbitrary quantity.' Of course this does not affect the principle, but it tends, as do other things pointed out by the Commissioners, to throw doubt upon the exactitude of the experiments on which so much is being made to depend. Taking the results of the experiments as they stand, however, the Commissioners come to the conclusion that inoculation sensibly diminishes the incidence of attacks on the inoculated population. But the protection afforded is not absolute; indeed, plague has attacked persons who have undergone inoculation as many as four times in the course of two years previous to their attack, and as many as 8 per cent. of the inoculated population may suffer from plague, as was the case in Bulsar. It is thus impossible to give a numerical expression to the protection afforded by inoculation. They also say that inoculation diminishes the death-rate among those inoculated, not merely from the attack-rate being diminished, but from the fatality of the attacks being lessened. As for the duration of such protection as is conferred, this, it is held, certainly lasts for a considerable number of weeks. On the general question, then, they recommend that under the safeguards and conditions of accurate standardisation and complete sterilisation of the vaccine, and the thorough sterilisation of the syringe in each case, inoculations should be encouraged wherever possible, and particularly among disinfecting staffs and the attendants of plague hospitals. Reading between the lines of this very modified praise and this very mitigated recommendation, one can see that the Commissioners are by no means imbued with any superabounding faith in the proceeding as hitherto carried out, true as the principles may be upon which it is founded."

From the official report (Plague Commission) commented upon in *The Advocate of India*, August 26, 1897, we find that in the Brahmapuri Plague Hospital in Bombay, out of thirty patients inoculated with Dr. Haffkine's anti-serum only ten survived. The best proof of the inefficacy of the inoculations was that the plague pursued its course in spite of them. Mr. Paget treats us to abundant statistics, but " the field of medicine," it has been well said, " is alas, strewn all over with the abandoned bones of statistical fiascos." The most statistic-ridden people in the world are the doctors. Dr. Laurie, giving evidence before the Plague Commission at Hyderabad, said, according to the report in the *Bombay Gazette*, December 22, 1898—

> " An examination of Haffkine's fluid shows that it is not a serum, but is a putrescent organic liquid, a putrid broth." " Nothing would induce me, since I have seen what it contains, to undergo inoculation with Haffkine's fluid, nor could I conscientiously recommend it to my patients, European or native."

Mr. Paget would have his readers believe that all the opposition to M. Haffkine's plague inoculations emanates from " lying " anti-vivisectionists, political agitators, and the like.

The *Medical Annual* for 1903 says (p. 536) :—

> " One of the dangers of a serum becoming contaminated was well illustrated by an occurrence during anti-plague inoculations in the Punjab in November, 1902, when some eighteen persons died after being inoculated against plague. It was found that the serum had become contaminated by the bacillus of tetanus, and that the unfortunate mortality was ascribable to that cause. This, however, in no way affects the general question of plague prophylaxis, but indicates the extreme care necessary in preparation."

IX

TYPHOID FEVER

Mr. Paget gives us some statistics tending to prove the value of Prof. Wright's preventive inoculations against typhoid.

They were largely employed on our soldiers in South Africa, but with so little success that Dr. H. M. Culliman, in a paper on "Inoculation as a Preventive against Typhoid Fever," read before the Royal Academy of Medicine, Ireland, is reported in the *Medical Press*, August 21, 1901, to have said :—

> "If one can credit the results as evidenced among the soldiers in South Africa, enormous numbers of whom were vaccinated [inoculated], it would appear to have been somewhat of a failure."

Drs. Fagge and Pye Smith's *Text-Book of Medicine*, vol. i. p. 156, dismisses the matter in a few words :—

> "Recently Wright's prophylactic treatment by anti-typhoid serum has been used on a large scale in India, and with satisfactory though not striking results so far."

Very violent symptoms were sometimes caused by these inoculations, and the mortality from the disease amongst our South African troops, who were in many cases presumably inoculated, was enormous.

X

THE MOSQUITO: MALARIA, YELLOW FEVER, FILARIARIS

1. Malaria

Mr. Paget's aeeount of the mosquito as an intermediate host between man and man of malaria and yellow fever is very interesting and instruetive, but so far as malaria is eoncerned the only speeies of lower animals employed to any extent in the experiments in question were mosquitoes and sparrows. Surgeon-Major Ronald Ross experimented on these in India. Mosquitoes fed on the sparrow with numerous proteosoma were found to have become the intermediate hosts for the malarial germ.

When these mosquitoes fed on healthy sparrows they infeeted them. When mosquitoes were allowed to feed on malarious patients and afterwards on healthy men who submitted voluntarily to the experiment, these persons contraeted malaria. These important researehes resulted, not in the diseovery of any cure for the disease, but it set men to work in making the breeding plaees of the mosquitoes unfit for their existenee, and in preventing them from attaeking people by preventing their aeeess to their bodies. Several brave men let themselves be bitten by mosquitoes infeeted with pure benign tertian parasites, and were in due time attacked by malaria. The remedy for the disease is, as before these discoveries, quinine, and no experiments on animals were the means of introducing that valuable specific to medical practice.

2. Yellow Fever

Although no very cruel experiments were tried on animals or man in the search for the cause of malarial fever, the same cannot be said about yellow fever. Mr. Paget, the object of whose book is to advocate the claims of the experimenters on animals, has several times given instances of the futility of the practice. Thus he quotes from Sanarelli the investigations of a number of American physicians who "after having uselessly attempted experiments on animals, experimented on themselves," and all without result.

Dr. Finlay was led by these failures to inoculate himself and six soldiers with infected mosquitoes. In 1896 Sanarelli discovered the *bacillus icteroides*, and said in the *Annals of the Pasteur Institute*, (Oct., 1897) :—

> "The preventive and curative power of the serum of the guinea-pig, the dog, and the horse, vaccinated against the *bacillus icteroides*, should be held as absolutely demonstrative in the case of animals."

The Professor, like most of his class, was too sanguine, he failed alike with animals and men. There are few darker records in the bloody chronicles of the vivisectors than the story of Sanarelli's terribly cruel experiments on human beings, which, he published in the *Annali d'Igiene Sperimentale* (vol. vii. p. 441, *et seq.*). He says :—

> "I have made experiments on five men. For reasons easy to understand, I have not made use of living cultures, but of broth cultures 15–20 days old, passed through a Chamberland filter, and for the sake of greater precautions, I have sterilized them by drops of formic aldehyde. On two men I tried subcutaneous injections, on the other three the injections were intravenous. I sum up the following from the journal of my observations."

Some of the inoculations under the skin did not produce such violent symptoms as did those into the veins. In the case of E. N., aged 20, a Spaniard, we learn that soon after the injections he became violently sick, he rejected all the milk which he had drunk.

> "At the same time, the patient is seized with a general disturbance in all his limbs; there is a violent and persistent pain in the lumbar region, which causes the patient to utter cries of distress and deprives him of even a single moment of rest."

This was but the beginning of the torment. The experimenter continues:—

> "Little by little the abdominal region also becomes painful. The slightest application of the hands on those parts hurts the patient to an intolerable degree. In the meantime the axillar temperature goes up without interruption. . . . At about midnight the febrile reaction ceases, and the next morning the temperature is almost normal. . . . But the patient feels very unwell, and during the night not only has been sleepless on account of his lumbar and abdominal pains, to which a violent

headache was added, but he has also been continually tormented by an irrepressible vomiting."

The next day, November 4, we read that :—

"The disturbance of all parts of the body becomes more intense; the patient complains of an inexpressible feeling of anxiety, which deprives him of any rest whatever, whilst shooting pains afflict the lumbar region with a distressing persistence."

We are told that :—

"The man tries several times to throw himself out of the bed, and the pain in the lumbar region torments him so much that he utters continual shouts of anguish."

On the following day the same symptoms continued, and the pain became "still more distressing" : —

"There is continual vomiting, although the patient, after the injection of toxin, has not been able to take any food whatever."

On November 5, Prof. Sanarelli tells us :—

"By means of sterilized pipes I make some exploratory perforations down to the liver and the kidneys, and with the mouth I draw from those parts some drops of fluid of which I avail myself to make cultures and microscopical researches."

The researcher says :—

"During the following night the patient improves to some extent, and after some days he recovers, but I discontinue my observations on him in order to give all my attention to what follows."

Then he gives an analytical and microscopical account of the patient's blood.

We have the full history of the other similar cases. The fourth experiment was made on N. Q., aged 35, a Spaniard. He was inoculated at noon on November 12 with the terrible virus, and shortly afterwards the same dreadful symptoms began to manifest themselves. He suffered from persistent vomiting, and violent headache, great distress and pain all over the body. At eight o'clock in the evening we read :—

> " Just then the patient is seized with a violent and restless fit of anguish, which gives rise to incessant lamentations. . . . His eyes become red, moist and bright, his pupils dilated, his look anxious and pathetic, and all this gives to the patient the appearance of a drunken man."

At five p.m. the patient became worse :—

> " Pains more acute than before are felt in addition to frequent paroxysms of delirium. He can scarcely answer our questions and insists on pointing out his forehead, his stomach, and his back as the most painful parts of his body."

Later on his " whole body is violently shaken by a fit of tremor."

> " During the night the condition of the man has become worse. Vomiting and diarrhoea were almost incessant. At daybreak we learn that the patient is ' completely prostrated.' "

The man lived for further experimentation. The researcher says :—

> "I make aseptically a blood-letting of about 30 c.c.m. of blood which is allowed to coagulate in sterilized vessels in order to obtain its serum. I make also, as in the preceding experiment, exploratory perforations, taking care that the operations be quite aseptic, and I extract from the liver and the kidneys a small quantity of liquid, which I hasten to scatter over various mediums in order to proceed to some microscopic investigations. At 8 p.m. begins to show some collapse."

"After some days he recovers."

We read that in the other case though the man was made very ill by the inoculation, the symptoms were less intense than in the preceding cases, and the patient "entirely recovered."

Sanarelli says :—

> "To show the meaning and the importance of the experiments which I have had the lucky chance of making upon human beings, it is not necessary to display any arguments. Whoever has observed personally some cases of yellow fever, or has acquired a knowledge of its symptoms by reading the text-books on the matter, will find that the experiments 3 and 4 represent exactly the typical cases of a microbe poison in yellow fever,"

and he concludes that he has thereby demonstrated the existence of a microbe poison in yellow fever.

Dr. Whitla says :—

> "Attempts have recently been made to apply serum-therapy to the treatment of yellow fever, but the results have been most unsatisfactory; the

serum has failed in the laboratory and at the bed-side."[1]

Mr. Paget's reference to these cases is very brief. He says (p. 275) :—

> "Except five inoculations, where evidence that the persons understood the risk incurred is unhappily wanting, it appears that no inoculation has been made save with the consent of the person inoculated."

Medical opinion is divided as to whether the *bacillus icteroides* is the cause of yellow fever or not.

Be this as it may, Mr. Paget says that "The use of Sanarelli's serum treatment has not gone far." Messrs. Durham and Myers, in their report on yellow fever, conclude that yellow fever is not due to parasites of the nature of protozoa, nor do they believe that the disease is carried by mosquitoes. (*Medical Annual*, 1903, p. 697.)

3. Filariasis

The treatment of this disease is preventive and consists in sterilizing the water supply and sleeping under mosquito-netting.

Mr. Paget concludes the section of his work which deals with the part played by the mosquito in causing disease with the following remarks :—

> "Thus, in a few years, from experiments on mosquitoes, sparrows, and men, has come the present plan of campaign against malaria, yellow fever, and filariasis ; that is, against *Anopheles* and *Culex*."

[1] *Dictionary of Treatment*, p. 1020.

We have nothing but praise for the hygienic work of cleansing the land and out-houses round the dwelling-places of men in mosquito-beset districts, there is no evidence that the terrible insect which causes the mischief suffers in the least by the experiments quoted, and it is not probable that the sparrows employed are to any appreciable extent caused suffering. It is not against a research of this beneficent character that anti-vivisectionists protest, but against the real vivisections and the tormenting sicknesses to which vertebrate animals in common use in the laboratories are daily subjected. Mr. Paget does but draw a red-herring across the scent when he classes the blood-sucking gnat with the tortured dog on the rack. He "pays tithe of mint and cummin and omits the weightier matters of the law," justice and mercy.

Rinderpest

Rinderpest is said to have been stamped out in South Africa in consequence of Koch's researches. But in *The Globe*, December 2, 1898, was published the following letter:—

> "Sir,—In your issue of *The Globe* of to-day I notice that Lord Lister, addressing a meeting of the Royal Society, stated 'that Drs. Kolle and Turner, after investigations based on the facts ascertained by Koch, had discovered a mode of treatment by injection which secured complete and lasting immunity from rinderpest, and in that way during the past two years the lives of more than 700,000 cattle have been saved.' A statement like this from such a source should be considered authentic, but as the information here conveyed is of such vital and over-

whelming importance to the well-being of cattle, and to the safety thereby imparted to the investment of capital in live stock, not only in this country, but in our colonies of South Africa and Australia, it would be well that further information should be acquired; first, as to the nature of this 'prophylactic' which is to render cattle immune from the attack of so fatal a disease; and second, as to where the 700,000 cattle were rescued from destruction; and third, as to the scientific proof that these cattle would otherwise have perished. Such information, satisfactorily authenticated, would raise the value of cattle farms in all parts of the world fully 50 per cent. It is not so long ago that Dr. Koch was reported to have abandoned all hope of saving the cattle stricken with rinderpest in South Africa; and certainly the general opinion is that his efforts in that part of the world had proved fruitless.—I am, sir, your obedient servant,

"A Ranch Man."

December 1.

XI

PARASITIC DISEASES

This section of *Experiments on Animals* need not detain me long. It is not concerned with bacteriology, the cutting into of living animals or other cruel laboratory work. It has no more to do with vivisection than has experimentalism with the itch, or the parasites that infest the hair of men and animals.

Animals have been fed with "measly" meat to produce tapeworms in them. Trichiniasis is caused by eating infected ham and pork. Hydatid disease is caused by an animal parasite transmissible between men and dogs. All these things have been studied by feeding experiments, and none of them, nor all taken together, would have caused the crusade against vivisection, because it is a sane movement and not a sentimental fad.

XII

MYXŒDEMA

Mr. Paget is perfectly justified in including Myxœdema in his book because it is a subject which has entailed many prolonged and very cruel experiments on animals, and it is very easy indeed for an apologist of vivisection to make it appear that the marvellous alleviative treatment of the complaint within the past few years was originated in consequence of these experiments. I have not the remotest idea of making light of the important researches made in this connexion; they have been of the greatest value to medical science, and have opened up the study of many important questions in relation to the various ductless glands of the animal body. But I venture to think that all that has been done for the relief of patients suffering from this disease could have been achieved by the practice suggested by Sir Charles Bell, which so excites Mr. Paget's contempt: "The observation of the first facts of anatomy and of natural motions."

When I was a student the use of the thyroid gland was not understood, and nobody could throw any light on the mystery of its presence in the body. I could not believe that the Creator had placed it where it is for no purpose whatever, and I was greatly interested in the discussion on its funetions whieh shortly afterwards took plaee. In 1877 Dr. Ord diseussed the relations of myxœdema to atrophy of the thyroid gland. He was on the right traek, there was no need of experiments on animals at all. It had been diseovered in the hospital at Berne that in eighteen cases of eomplete removal of enlarged thyroid gland a eondition whieh we now know as myxœdema followed. When the gland is removed from a human being he becomes a cretin. This should have suffieed to prove to our pathologists that the thyroid body had some important offiee to fill in the animal organism.

Then again the chemists should have been set to work. Baumann, of Friburg, suggests that an absence of iodine is the cause of the trouble.

> "At any rate, iodine is to be found in the normal thyroid secretion in close eombination with albumen, whilst it is absent in eases of goitre, the enlargement of the gland being looked on in the light of a eompensatory hyperplasia."[1]

Had ehemists done at the outset of the inquiry what they have sinee done in their laboratories, there would have been no necessity for the many cruel experiments on animals whieh have been performed in this eonnexion. It is a remarkable thing that so long ago as 1863 iodine was the best treatment for thyroid disease and eretinism, and thyroiodin or the iodine naturally produeed by the healthy gland is the treatment of to-day.

But the routine method by experiment on animals is the vogue in the laboratories, and it was easier to

[1] *A Manual of Surgery*, Rose and Carless, p. 778.

vivisect than to analyse, and withal made more noise. "Take a dog" is as familiar a phrase in physiological treatises as "take a cabbage" in a cookery book, and so the vivisectors took animals of all kinds—rabbits, sheep, calves, horses, dogs, cats, foxes and monkeys.

In 1884 Sir V. Horsley was able to produce the disease in monkeys, causing "a cretinoid state, the facsimile of the disease in man." But this had been already discovered by surgical operation in human patients at Berne hospital long before. I am told by a vivisector that in a certain laboratory where experiments on various animals were in progress in connexion with this research, a number of dogs from whom the glands had been experimentally removed were rapidly wasting, whilst other dogs in the laboratory who had suffered a similar mutilation were regaining their health in a way that puzzled the experimenters, until it was discovered that the latter animals had been devouring the thyroids which were thrown in a corner after removal from them and their companions first operated upon. I am assured that this accident suggested the new treatment by administering to myxœdematous patients the thyroid glands or the extract therefrom of animals killed at the butchers.

In 1890 Sir V. Horsley, following the suggestion of Schiff and Von Eiselsberg, published the proposal that thyroid tissue taken from an animal recently killed should be transplanted beneath the shin of a patient suffering from myxœdema.

Then Dr. Murray found that hypodermic injection of thyroid extract would answer equally well in relieving the patients, and afterwards tabloids of the extract were found to be not less successful. Better still, the isolation of the active principles of the gland in the form of thyroiodin promises equally good results in treatment. Considering all these facts, I cannot agree with Mr. Paget that any experiments on animals were necessary in this research, profoundly interesting and valuable as it undoubtedly was. For first, we knew that disease of the thyroid produced severe illness, and its removal from hospital patients caused myxœdema. Had chemists done at this point what they have done since they would have discovered the iodine in the gland, which had already been found by chemical observation useful in the treatment of the disease. Had the physiologists suggested the administration of the thyroid glands of the sheep taken from the slaughter-houses to the patients the whole business might have been settled without a single experiment on a living animal.

XIII

THE ACTION OF DRUGS

PERHAPS the weakest part of Mr. Paget's work is the chapter on the Action of Drugs. He says (p. 300) :—

> "It was the physiologists, not the doctors, who first formulated the exact use of drugs; it was Bichat, Majendie and Claude Bernard."

And what have the physiologists done in this direction? After their myriad experiments on the action of drugs, what have they given us? Majendie, says Mr. Paget, experimented on the action of the upas-poison and on strychnine and Claude Bernard on curare and digitalis. As upas-poison has no therapeutic use of which I am aware, and as curare is only used by vivisectors to keep animals quiet whilst experimenters torture them, these studies of valuable drugs are confined to two, strychnine and digitalis, and this, because according to Mr. Paget, they revealed the "*selective*" action of drugs. Now it is all very well for the physiologist to take a potent drug and tell us to which precise organ or part of the body it will travel when it enters our system and so help us to treat our patients on pure physiological principles, but this is very far indeed from therapeutics, as practising physicans understand the art. We cannot treat one organ or one set of nerves to the exclusion of the rest of the body. We are men and women, not packages of kidneys or bundles of blood-tubes. Of many drugs it can be said, as Drs. Stillé and Maisch say of Sanguinaria (Bloodroot), a remedy for bronchitis: Its "physiological action," as shown by many experiments on animals, "bears no relation to its medicinal use." (*National Dispensatory*, p. 1,254.)

Take a common domestic medicine like squill, the same authors say (p. 1,279) :—

> "There is nothing in the results of scientific investigation even to suggest that squill acts upon the bronchial mucous membrane, but the much more direct and conclusive evidence of clinical experience leaves no doubt of its great value in bronchitis."

Concerning Woody Nightshade (*Solanum Dulcamara*), Stillé and Maisch say (*op. cit.* p. 519) :—

> "The so-called scientific therapeutists of the present day are disposed to deny any curative virtues to dulcamara, because they are unable to explain those it is alleged to possess, according to their notions of the mode of action. Such a reason may, in a logical sense, be called impertinent. The claims of dulcamara rest on the same grounds as those of opium, mercury, and cinchona, the ground of clinical experience."

Sir Lauder Brunton, M.D., in his work entitled *A Text-Book of Pharmacology, Therapeutics, and Materia Medica*, says (p. 39) :—

> "Almost all our exact knowledge of the action of drugs on the various organs of the body, as well as the physiological functions of these organisms themselves, has been obtained by experiments on animals."

Medicine is not an exact science, and the most accurate acquaintance with the action of a drug upon a particular organ of the body very often indeed fails to assist the physician in curing the malady from which that organ may be suffering. Again, many of our most valuable remedies have been used in medicine for long ages before we come to know their precise physiological action, and if the doctor had to pause to-day before prescribing for his patients, to settle in his mind what is the precise physiological action of his remedies, his patient would often die or recover before he had finished the solution of the problem. Quinine, for example, was used for the cure of ague and malarious fevers some two hundred years before the bacteriologists discovered how it acted in such cases. If the Jesuits who discovered the properties of cinchona bark in the forests of Peru, had neglected to use it as a medicine until scientists had found out what it did when taken into the system, they would have deprived the world of the benefits of Peruvian bark from the year 1628 till 1880, when Laveran discovered the small organism to which malaria is due.

There are few poisons or potent drugs which act upon animals exactly as they act upon man. Many animals, especially the herbivorous, eat with impunity poisons deadly to us, and conversely the domestic saline called "Mindererus Spirit" is a deadly poison to rabbits; camphor induces in birds epileptic convulsions, to mammals it is intoxicant; lemon juice is a powerful poison to cats and rabbits; extract of meat is fatal to dogs when injected into the stomach; glycerine given subcutaneously poisons dogs.

Mr. Paget says of—

> "Aconite, belladonna, calcium chloride, colchicum, chloral, ergot, morphia, salicylic acid, strophanthus, the chief diuretics, the chief diaphoretics—all these drugs, and a host more, have been studied and learned (*sic*) by experiments on animals."

If by the drugs having been "*learned* by experiments on animals," we are to understand that more or less interesting facts have been observed by administering them to animals, I shall not dispute the matter, but if by "learned" it is intended that physicians have learned their medicinal properties by experiments upon animals alone, I oppose the thesis. On all questions concerning the action of drugs the experimenters on animals are at open warfare. Their Babel voices have turned the pharmacological laboratory into "a city of confusion," it is there, if anywhere, that the art of proving your colleague a tarradiddler is carried to perfection.

Concerning the action of aconite Achscharumow is contradicted by Lauder Brunton, Böhm, Wartmann, and Wood. Ringer and Murrell deny the accuracy of the experiments of Liégeois and Hottot. Of the conclusions of MM. Gréhaul and Duquesnel Wood says:—

> "That their results are so strikingly different from those of other experimenters as to indicate the existence of some fallacy."

Dr. Ringer says: "The views concerning its action on the nervous system are very diverse."[1]

Concerning belladonna Drs. Stillé and Maisch say:—[2]

> "The physiological action of belladonna, as revealed by experiments, is far from pointing clearly to one of the most useful applications of the drug, in *relaxing spasm*, which has long been known as a clinical fact."

Meuriot and Harley contradict each other upon the results of their experiments with the drug. Wood says that none of the experiments seems decisive, and that their results are not in accord with clinical experience. On other points Dr. Erlenmeyer is opposed by Brown-Séquard and Harley. Dr. Ringer says on this: "It must be remembered, however, that these drugs do not similarly affect animals and man." Of colchicum it was only within the first quarter of the last century that its use was revived, after the discovery that the virtues of an unquestionably efficient quack remedy for the cure of gout, were due to colchicum. Stillé and Maisch say of gout, the disease for which it was anciently employed, the drug only

> "Fell into neglect through the prevalence of absurd medical theories, which condemned it because their authors could not comprehend its operation."[3]

Concerning calcium chloride, Sir Lauder Brunton says it "is not much used in medicine" (*Pharmacology*,

[1] For my authorities on all this see my *Futility of Experiments with Drugs on Animals*, p. 7.

[2] *National Dispensatory*, p. 278.

[3] *Ibid.* p. 442.

p. 582), and Stillé and Maisch say its use "is not unattended with risk." All the alkaloids and neutral bodies derived from opium act differently on man and animals, and the statements made concerning their action by experimenters are very conflicting. It would be difficult to harmonize the conflicting results which experimenters have arrived at from their researches with salicylic acid on animals.[1]

Paul Bert experimenting with cocain asserted that its action is purely local, but this does not correspond with the experience of others. The researches of Merino do not bear out the assertions of Liebrich as to the results of experiments with butyl-chloral. With regard to the action of digitalis the experimenters Boehm, Schmiedeberg and others are in opposition to Ackerman and Brunton. Ringer says that according to Saunders, Jôrg, Hutchinson and others, digitalis in moderate doses in the first instance, quickens the pulse, though other observers deny this effect."[2]

Of ipecacuanha, notwithstanding its enormous use and the great number of experiments upon animals made with it, we may sum up the results in the words of Dr. Wood :—

> "Its physiological action is not yet well made out."

Of hemlock the results of experiments on animals are so conflicting that Dr. Stillé says :—[3]

> "These antagonistic results of experiments conducted under determinate conditions illustrate the

[1] *National Dispensatory*, p. 75.
[2] *A Handbook of Therapeutics* (5th ed.) p. 411.
[3] *Therapeutics*, p. 431.

difficulty of drawing definite conclusions from such data and the wisdom of preferring clinical bases for clinical rules."[1]

Concerning ergot Wood says[2] that the physiological observations of Holmes and of Wernick on the action of ergot on the circulation are directly contradicted by Dr. Paul Vogt, who experimented on rabbits, and the results obtained by Eberty are in accord with those of Vogt, and disagree with those of Holmes.

Of strychnine, Stillé says :—

> "Although physiological experiments do not lead to the suggestion that strychnine acts upon the peripheral ends of nerves, clinical observation, as in so many other cases, is supposed to demonstrate what the former method has failed to show."

This is a very important admission emanating from a great authority on Materia Medica, and tends to prove that we are not retarding the progress of medical science by our efforts to confine it to its proper sphere.

Professor Bouchard, in a paper which he read at the meeting of the Medical Congress held at Cairo in December, 1902, said : —

> "Empiricism has given us opium, which does not often cure, but which relieves almost always, and the empiricism of olden days has given us nearly all our drugs, among which are several which cure, such as quinine, mercury, the iodides, arsenic, colchicum, and salicin—all drugs the use of which we have learnt by happy accident. Each of these

[1] *National Dispensatory*, p. 456.
[2] *Therapeutics*, p. 546.

> drugs cures a special disease, and almost exclusively that disease, its action being specific. Our forefathers did not know or even suspect the reason, but most of us to-day know the secret of their action. They influence the poison by means of constitutional treatment."[1]

All that the experimenters have done for us is to tell us that in consequence of their prolonged and elaborate researches we may properly use these remedies for the diseases which have been treated by them successfully long years before the pharmacologists came upon the scene.

> Mr. Paget says that part of our contention is "That drugs act differently on animals and men," and adds "that the few instances that give a wise air to this foolish answer, were known long ago to everybody."

If this cryptic utterance means anything at all it implies that everybody who knew this was foolish for knowing what is an admitted fact, or was foolish for saying so. If the facts are as widely known as Mr. Paget would apparently wish us to believe, the majority of medical men whom I have met have taken the greatest pains to conceal their knowledge of the subject.

A surgeon of Regent's Park wrote to the *Lancet* a few years ago to say that he had discovered that morphia does not poison fowls. At the request of a lady patient who wished to destroy some pet fowls, he gave them—six hens and a cock—one drachm of acetate of morphia in bread and milk sop, the whole of which they swallowed. The effect was *nil*. Two days later the birds

[1] *The Lancet*, February 7, 1903.

were alive and strong. He mentioned in his letter that pigeons will eat large quantities of opium without experiencing any ill effect.

I fancy that this gentleman's acquaintance with the action of the drugs of the Pharmacopoeia on animals fairly represents the toxicological attainments of the average physician and surgeon on such matters, and I believe that very few indeed would lay claim to the possession of the rather useless information with which Mr. Paget credits them.

"Anæsthetics," says our author (p. 305), "must be reckoned amongst the drugs that have been studied on animals; but for the discovery of them, men experimented on themselves." Exactly! Had Dr. Simpson been guided by experiments on animals in his use of chloroform, we should not have had his discovery.

> "Flourens, the eminent French physiologist, tried the effect of chloroform on inferior animals, and, in consequence of its powerful and fatal influence on them, put it aside as an anæsthetic." [1]

[1] *Biological Experimentation*, by Sir B W. Richardson, M.D., F.R.S., p. 54.

XIV

SNAKE VENOM AND SUNDRY RESEARCHES

MANY savage tribes proteet a patient from the dangers of snake bite by making him suek a few drops of venom and eat some part of the snake's anatomy. Serpent venom taken by the mouth aets very mueh less violently than when injeeted either by syringe or bite. All this knowledge is as old as the hills amongst savages. Fraser's method of administering small doses of serpent poison to animals, and his diseovery that they rapidly eame to bear lethal, and finally enormous doses without hurt, was no novelty in medieine. But the diseovery that the blood of animals so treated beeomes an antidote against the fatal effects of snake poison in man is new and may be of great importance. Moreover, Dr. Fraser claims that the serum produeed through cobra venom injeeted into animals makes them proof against the bites of all snakes. But this is ehallenged by other experimenters, and Dr. Kanthaek, experimenting for the Loeal Government Board, eame to the eonclusion that eobra venom serum is not effeetive against the bites of all snakes.

Calmette, says Dr. Whitla, urges the importance of thorough ligaturing of the limb and irrigating the wound freely with a fresh solution of good bleaching powder in addition to the injection.[1] Now it is a remarkable fact that the Chinese immediately adopt the ligature when bitten by a snake, and Mr. Cantlie, F.R.C.S., says it "certainly is a fact that but few, very few, Chinese die of snake-bite."[2]

Thus between the practice of savages and that of the Chinese there was not much room for modern discovery in this connexion by experiments on animals.

The chief difficulty in the way of success in the use of anti-venene inoculations is the rapidity with which snake poison kills and the length of time that must usually elapse before a person bitten in India can avail himself of treatment by these inoculations.

In concluding the second part of his book the writer says: "Nothing has been said of the many inventions of medical and surgical practice that owe only an indirect debt to experiments on animals." He enumerates the following subjects on which he might have had something to say:—

Researches on the Suprarenal Glands and Adrenalin

Dr. Osler says:—[3]

> "Schäfer and Oliver have shown that the human adrenals contain a very powerful extract; they have also studied the toxic effects on animals of the extracts of these glands."

[1] *Dictionary of Treatment*, p. 878.
[2] *Medical Annual*, 1898, p. 495.
[3] *Principles and Practice of Medicine* (2nd ed.), p. 747.

It is to be noted that the active principle of these glands was discovered from testing the glands of dead human beings. Experiments on animals followed the discovery. There is much discussion still amongst physiologists as to what it is if the glands contain a true active principle.

Dr. Hunter's work on Pernicious Anaemia is mentioned. This observer made important suggestions on this disease, but says Dr. Whitla:—[1]

> "Though much light has in recent years been thrown upon the pathology and diagnosis of this formidable affection by the brilliant researches of a host of observers, there is little advance in its treatment."

Artificial Respiration, when practised on human beings, is best undertaken by Dr. Sylvester's method, and this owes nothing to experiments on animals, but depended on observation on human bodies, and experience in their treatment. Many cruel experiments on animals were performed under the auspices of the Royal Medical and Chirurgical Society, but they led to nothing. Transfusion of Saline Fluid was adopted in place of the old method of transfusion of blood, of which Mr. Lawson Tait said he had "seen it performed seven times without success in a single instance." The Infusion of saline fluid—the injection of a solution of salt into depleted veins, if not as old as Adam, is at least as old as the cholera visitation of sixty years ago.

The Twisting of Arteries for arrest of bleeding was practised by Rufus of Ephesus (A.D. 98-117), so that Mr. Paget cannot claim the discovery for any of his

[1] *Dictionary of Treatment*, p. 43.

physiological heroes. The Grafting of Skin was not a discovery due to vivisection. "The skin of animals," say Messrs. Rose and Carless, "such as frogs and young rats, has been employed with success in some cases; but it is just as easy, and much more satisfactory, to make use of human skin for this purpose."[1]

Transplantation of Bone. We have read of wonderful cases where portions of the bones of living animals have been used to repair damages in human limbs and heads. I have never seen the operation, nor do I believe it is successful; but even more wonderful transplantations are recorded. It is reported in Sir John Lubbock's *Prehistoric Times* that the Society Islanders are great surgeons.

> "On some occasions, when the brain has been injured as well as the bone, they have opened the skull, taken out the injured portion of the brain, and, having a pig ready, have killed it, taken out the pig's brains, put them in the man's head, and covered them up."

No doubt in all such cases the "covering up" would be the most important part of the business.

The Therapeutic Uses of Electricity have had nothing to do with vivisection, so far at least as Finsen's Light Cure for Lupus and the Röntgen Ray treatment of Rodent Ulcer are concerned. Mr. Paget mentions the hypodermic administration of drugs, the use of oxygen for inhalation, and the rational employment of bleeding as in some remote way or other connected with his subject. He might have added the use of fresh air, pure water, and warm clothing as practices that "owe an indirect debt to experiments on animals."

[1] *Manual of Surgery*, p. 69.

PART III

ANÆSTHETICS USED FOR ANIMALS

Mr. Paget assures his readers that animals take ether well, "and that there is no difficulty in rendering them unconscious with it" (p. 343). He adds that with some animals chloroform is equally good, though for dogs and cats ether is better. "And it is wholly false to say that 'just a whiff' of chloroform or ether is given, or 'just enough to keep the animal quiet.'" Let us test the truth of these statements.

In the issue of the *Journal of Physiology* for March, 1903, Vol. XXIX., No. 2, there is a paper by Drs. Brodie and Dixon, on "Bronchial Muscles," on p. 144, of which we note the following:—

> "In studying these reflexes we have found it of the utmost importance to avoid the use of chloroform or ether as the anæsthetic. The experiments must therefore either be performed upon unanæsthetised animals, upon animals anæsthetised with morphia, or upon decerebrate animals [that is, animals whose brain had been removed]. Our experiments were usually conducted under one of the two latter conditions, but in a few instances were repeated upon animals lightly anæsthetised with chloroform. In no instances have we obtained any effect by exciting the central end of the sciatic

> in a curarised animal, but the animals are not numerous."

Here we are told that sometimes the experiments were performed upon animals "lightly anæsthetised with chloroform," but in a previous sentence it was declared that it was of the utmost importance to avoid the use of chloroform and ether as the anæsthetic. We are to conclude, therefore, either that the experiments were worthless, or that the administration of the chloroform was the mere "whiff of chloroform" which "keeps the word of promise to the ear" of the Home Office "and breaks it in our hope."

In a paper on "Gravity and the Circulation," in the *Journal of Physiology* (vol. xxi., p. 323) by Professor Leonard Hill, we read :—

> "It is absolutely essential that chloroform should not be administered during the periods of observation, for there is no other agent known to us that so rapidly abolishes the mechanisms which compensate for the influence of gravity as chloroform."

The cutting operation is not always the most painful portion of the experiment, and is usually quickly completed. The period of observation may last for many hours and entail the greatest torments. We are told (p. 343) that Professor Hobday has pointed out that "the lower animals can be most successfully given chloroform if they are properly dealt with." But this same eminent veterinary surgeon, it appears, is not always successful even when he himself deals with the animals. In an article in the *British Medical Journal* (March 7, 1896) Mr. Sydney Rowland, of St. Bartholomew's Hospital, tells how he had made several attempts to obtain a skiagraph of a pregnant cat. We learn that

> "There was some difficulty in keeping the cat at rest, and an anæsthetic was rendered impossible by the fact that during a trial with ether and another with chloroform the cat showed signs of danger, so much so that Mr. Hobday, of the Royal Veterinary College, who kindly managed the operation, and at whose suggestion the cat was tried, deemed it inadvisable to proceed."

There is overwhelming evidence to the effect that drugs such as morphia and chloral are largely used in place of true anæsthetics, because of their greater safety. We constantly read in reports of experiments "anæsthetic—morphia" Mr. Paget defends the use of this drug for keeping the animal quiet during experiment. But the action of morphia upon animals is a very variable one. "Every species of animal has an impressionability very different," says Dr. Vibert (*Précis de Toxicologie*, p. 630), and whilst the horse finds 7 milligr. per kilog. a toxic dose, the cat requires 40, and the dog 65 milligr. to affect the creature similarly. Sometimes acting as a violent stimulant, at other times producing vomiting and purging, the dog, although narcotized by a large dose of morphia, "feels the pain, but has lost the idea of self-defence," as Claude Bernard says. If opiates could have been employed as anæsthetics in the days when there was neither chloroform nor ether, why were patients compelled to undergo the unspeakable agony of major operations without their help? No operations calculated to cause pain in animals ought to be permitted where the use of true anæsthetics such as chloroform and ether is impracticable. Surgeons do not operate on human patients with sham anæsthetics like morphia or chloral.

"Curare," says Mr. Paget, "is not an anæsthetic under the Act." Of course it is not, but that does not interfere with its regular employment in the laboratory when, in the words of our author, "For the purposes of the experiment—to put the matter on the lowest grounds--the animal must be kept at rest" (p. 344). Curare does this to perfection. Says Mr. Paget's hero of physiology:—

> "Curare is now employed in a vast number of experiments as a means of restraining the animals. There are but few observations of which the narrative does not commence by notifying that they were made on a curarized dog" (*Physiologie Operatoire*, p. 168).

Mark also the following:—

> "Curare is a drug which has important uses in a certain class of experiments upon animals. It has never been claimed by any scientific man that it is an anæsthetic. Its use has led to important physiological discoveries which could not well have been made without it, and in a limited class of cases its employment, either with or without the coincident addition of anæsthetics, is indispensable."—*Semi-Centennial of Anæsthesia*, by Dr. W. H. Welch, p, 67.

In a long paper in the *Journal of Physiology* (July, 1893) by Dr. G. W. Stewart, there are records of a great number of experiments on dogs and rabbits performed in connexion with researches on the circulation of the blood. The vivisections, in these experiments, consisted in cutting through the belly for about three-quarters of its length, drawing out the intestines, separating and partly severing the kidney, opening the chest, by cutting through the ribs, ligaturing nerves, stimulating them by electricity, stimulating the spinal cord, performing tracheotomy, and so forth. On p. 71 of this paper we read that

> "In all the experiments performed in the physiological laboratory at Cambridge, and in most of those done in the Physiological Institute at Strasburg, the animals were anæsthetised with chloral, morphia, urethan, chloroform, or ether.
>
> "When curare was given, it was generally in addition to one of these anæsthetics."

The three drugs first mentioned are, of course, not anæsthetics at all, and it is to be noted that curare when given was generally employed in addition to one of the so-called anæsthetics. Let us examine this more closely. Dr. Stewart says on p. 8 :—

> "Since chloral lowers the blood pressure, it might seem a bad narcotic for experiments on the circulation. But for comparative experiments, where a notion of the relation between the circulation time in different organs is wanted rather than absolute measurements, it is a good drug, as it may be supposed, to cut out accidental variations of the calibre of the smaller vessels. In all experiments with section and stimulating of nerves, curare or urethan was used."

Classifying the experiments, we find that:—

Chloral was the drug employed in			7	cases
Chloral and curare	,,	,,	1	,,
Morphia	,,	,,	2	,,
Morphia and curare	,,	,,	3	,,
Curare alone	,,	,,	10	,,
Curare and atropine	,,	,,	1	,,
Nothing at all	,,	,,	1	,,

If, in any period of the experiments, chloroform or ether was given, there is no record of the fact. One experiment lasted six hours and twelve minutes under curare, another lasted close upon five hours. We remark such observations as the following where curare was used. "Artificial Respiration." A dog was "put under morphia for operation." "20 millig. curare into blood, as animal could not be got properly under morphia."

In the *Journal of Physiology* for September, 1893, there is a paper by Mr. W. Townsend Porter, in which the writer says (p. 127):—

> "Dogs were used in my experiments. The second, third, fourth, and fifth dog of the series of thirty-two recorded here were given a small quantity of morphia. Voluntary movements were prevented by curare."

In these cases the chest was opened and the heart reached through the opening. The time occupied by these vivisections varied from 18 to 100 minutes.

In his desire to show that curare paralyzes not only motor, but also sensory nerves, quotations are given in Mr. Paget's book with a view to prove this. A case is mentioned (p. 347) where a servant wounded her arm with a poisoned arrow, and became collapsed. Artificial respiration was kept up, and this with other treatment restored the patient. The girl declared that she had felt nothing of the operation of excising the wound, having been unconscious from within half an hour of the accident. Now there is no evidence whatever that the arrow was poisoned by curare. Savages use other poisons than this, and even curare itself is not a pure drug, but is a compound, and nobody knows exactly what are the plants employed to prepare it, though it appears certain that they derive their toxic properties chiefly from the strychnine family.

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